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**THE WHITE DWARF AFFAIR:**  
**Chandrasekhar, Eddington and the Limiting Mass**

by

**Sakura Gooneratne**

**University College London  
University of London**

**A thesis submitted in partial fulfilment of the  
requirements for the degree of**

**Doctor of Philosophy in the History of Science**

**2005**

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## **ABSTRACT**

### **The White Dwarf Affair:**

### **Chandrasekhar, Eddington and the Limiting Mass**

by Sakura Gooneratne

A thesis describing and analysing the controversy between Subrahmanyan Chandrasekhar and Arthur Stanley Eddington over the limiting mass of white dwarf stars. The aim of the thesis is to discover why the controversy occurred and to analyse the reasons behind Eddington's rejection of relativistic degeneracy and the limiting mass. The ultimate reason behind Eddington's attack on relativistic degeneracy was found to be Eddington's severe objection to singularities which was apparent long before Chandrasekhar's discovery of the limiting mass and occurred in three separate areas of research undertaken by Eddington during this period: astrophysics, cosmology, general relativity and Dirac's relativistic equation of the electron which led to Eddington's fundamental theory. The thesis will focus on the problem of the limiting mass of white dwarfs between 1929 and 1935 but will use the problem to analyse Eddington's view of singularities within the three different research areas spanning two decades from 1916 to 1936.

The Chandrasekhar-Eddington controversy is set within Eddington's earlier controversies with James Jeans and Edward Arthur Milne who together with Eddington founded theoretical astrophysics during the 1920s. The thesis will examine the problem of white dwarfs within the context of the earlier controversies on stellar structure. As well as the technical analysis of the controversy, the thesis will also analyse the social dynamics and interactions within the astronomical community and their impact on the controversies.

The aim of this thesis is to create a more complete picture of the Chandrasekhar-Eddington controversy by analysing Eddington's arguments for rejecting relativistic degeneracy, the limiting mass of white dwarfs and singularities not just within the context of astrophysics, but also cosmology, general relativity and quantum mechanics and to provide some new explanations as to why Eddington opposed relativistic degeneracy.

## **Acknowledgements**

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## INTRODUCTION

### Aim

The aim of this thesis is to describe and clarify the controversy over the limiting mass of white dwarf stars between the astrophysicists Subrahmanyan Chandrasekhar and Arthur Stanley Eddington which occurred in 1935, but whose repercussions were felt for the next thirty years. I will try to show that Eddington's rejection of the Chandrasekhar Limit is due to his absolute instinctive rejection of singularities. The reasons behind the controversy range from purely mathematical to conceptual, and perhaps even psychological, and although the thesis will not give a definitive explanation for Eddington's behaviour, an attempt will be made to assess the events leading up to the controversy and to follow the astrophysicists through and beyond it.

The thesis will cover the birth of theoretical astrophysics, the early astrophysical controversies between Eddington, Jeans and Milne, research on white dwarfs and the limiting mass, the controversy between Chandrasekhar and Eddington, its aftermath and, finally, an analysis of the reasons why the controversy occurred with respect to Eddington's stance against relativistic degeneracy and his views on singularities. The focus of this thesis is the Chandrasekhar-Eddington controversy and the events which surround it from its inception within the Eddington-Jeans-Milne controversies to its conclusion with Eddington's death. In order to give a comprehensive and detailed analysis of the controversy, I will also discuss not only the technical aspects of the controversy but the sociological, psychological and geographical dynamics inherent within the astronomical community during the interwar years. I will also assess claims of racial discrimination against Chandrasekhar and the impact of Eddington's religious faith on his scientific stance.

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## Scientific controversies

Scientific controversies have widely been used to examine a variety of themes in the history, philosophy and sociology of science because it exposes the mechanisms of scientific research which are normally closed to the non-scientist. There have been many attempts at trying to define and deconstruct the nature of scientific controversies and the orthodox versions generally are philosophical studies which tend to focus on the technical nature of controversies (internal science).<sup>1</sup> The majority of scientific controversies are resolvable by technical means such as comparison of data and method but those that cannot be resolved in this way tend to be over interpretations, cognitive aims and aesthetics. Controversies of this type can be resolved by scientific consensus but their subjective nature often means that they may be influenced by external factors, (such as status and authority) to some degree.<sup>2</sup> The Chandrasekhar-Eddington controversy falls into this category because it was not over the method of calculation but the understanding of the principles behind the theories used and could not be resolved by comparison of data or method. The difference was not due to whether one theory was better than the other, but on the incompatibility of their understanding of the same theory.

Controversy studies have previously been dominated by philosophers whose definitions were felt by many historians and sociologists to be restrictive and there has been a concerted effort amongst sociologists and historians to redefine and deconstruct controversies to provide a more naturalistic explanation of how science actually is

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<sup>1</sup> See Machamer, Pera and Baltas (eds)(2000) and Engelhardt and Caplan (eds)(1987) for articles on the subject of controversy studies including definitions of structure and closure. The articles generally are slanted towards a philosophical, rather than sociological, interpretation.

<sup>2</sup> Laudan (1984): 27; Engelhardt and Caplan (eds.)(1987): 13.



produced. Not only are they focussing on the technical aspect, but they emphasise the importance of social or external factors which may influence controversies. The majority of sociological studies such as by David Bloor, Harry Collins, Steven Shapin and Simon Schaffer however have concentrated on experimental rather than theoretical science and Andrew Warwick has tried to rebalance this by arguing that it is possible to write a cultural history of theoretical physics by employing the same analytical tools.<sup>3</sup>

Martin and Richards have identified four approaches to controversy analysis: positivist, group politics, constructivist and social structural. But as many scholars have found, controversies rarely fall into any one type of category and these are considered 'ideal types' of analyses.<sup>4</sup> The positivist approach begins with the acceptance of the orthodox scientific view and analyses the controversy from there, questioning why there is a controversy when the scientific issues are straightforward. The focus is on examining the critics of orthodox views. The group politics approach discusses how different groups approach scientific issues (this is mainly for public policy issues). The constructivist approach or sociology of scientific knowledge (SSK) challenges the positivist approach and applies a social analysis to scientific knowledge claims. The main difference is that both sides in the controversy are examined using the same repertoire of analytical tools (principle of symmetry).<sup>5</sup> The social structural approach uses class, state and patriarchy to analyse society and provide insights into controversies mainly between people and groups, for example, Marxism and feminism.<sup>6</sup>

In the case of the Chandrasekhar-Eddington controversy, the two main analytical tools that would be appropriate are positivism and constructivism. Positivism, normally

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<sup>3</sup> See Bloor (1976); Collins (1985); Shapin and Schaffer (1985); Warwick (2003): 11-16.

<sup>4</sup> Martin and Richards (1995): 507.

<sup>5</sup> Martin and Richards (1995): 513; Golinski (1997); Bloor (1991).

<sup>6</sup> Martin and Richards (1995): 509-514.

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the domain of philosophers such as Popper and historians such as Merton and Kuhn, has been the conservative tool of historians of science and rely on the premise that scientific theories are accepted when facts are proved. This is often described as ‘internal science’ and seen by many as a ‘sociology of error’ where ‘those who are wrong are analyzed to find out why’ tends to focus on the technical or internal history of science.<sup>7</sup> In *The Structure of Scientific Revolutions* Kuhn introduced the concept of paradigm change, where periods of normal science are interjected with episodes of violent revolutions when scientific world views are overturned, as a cyclical model of science. But he is careful to keep his analysis concentrated on the technical nature of scientific research and does not look for external factors which may have influenced the mechanism of scientific research. SSK or constructivism, a more recent methodological tool formulated by sociologists dissatisfied with both the Popperian and Kuhnian interpretation of science, places more emphasis on the social or external factors which influence the construction of scientific knowledge without neglecting the importance of the technical content. Scientific knowledge here is seen as socially constructed and is a product of social processes and negotiation. There are a number of schools of thought, notably the ‘Strong Programme’ introduced by Bloor in *Knowledge and Social Imagery* which emphasises causality, symmetry, impartiality and reflexivity when examining scientific controversies. Bloor believes that science is not solely driven by social factors but there will always be some inherent influence.<sup>8</sup> Collins, on the other hand, in *Changing Order* shows that social factors are paramount in his studies on tacit knowledge in scientific training and the replication of scientific experiments. He has shown that scientific controversies that were thought to be resolvable by comparison of

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<sup>7</sup> Martin and Richards (1995): 509.

<sup>8</sup> Bloor (1976): 5-8.

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observational data were not as straightforward as they seemed because replicating successful experiments to verify results was harder to achieve than previously thought and was strongly influenced by training and the scientific community and location in which the experiments were conducted. Collins also describes the scientific community as comprising a 'core set' of scientists involved in the creation and controversy of a theory surrounded by scientific onlookers which would influence closure and consensus on certain theoretical and factual interpretations.<sup>9</sup>

In his survey on constructivist thought *Making Natural Knowledge*, Golinski draws attention to the issues of authority, discovery, religion, geography and pedagogy which also contribute greatly in the construction of scientific or 'natural' knowledge. His emphasis is on scientific knowledge being locally created, produced and situated. As well as the works of Bloor and Collins, he also discusses Bruno Latour, who in his studies on Pasteur and laboratory life claims that there is no division between internal and external science; they are not mutually exclusive.<sup>10</sup> Although working within the same framework, Bloor is not so heavy handed; he does not claim that science is completely socially constructed but that one cannot examine the construction of scientific knowledge without bringing in some social factors. Shapin and Schaffer's *Leviathan and the Air Pump* examines the importance of authority stemming from social class and educational background and the cartography of knowledge where location and authority is intimately connected when practicing science using the case studies of Thomas Hobbes and Robert Boyle. For them, controversies provide two ideal scenarios to study events in science. Scientists involved in controversies question the reality and existence of facts and theories and methods of practice which will later become an

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<sup>9</sup> Collins (1985): 56, 142-145.

<sup>10</sup> Golinski (1998): Chp. 1.

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accepted part and practice of the field, and the scientists themselves deconstruct their opponent's beliefs and practices, as well as constructing their own arguments, providing the historian with ample material to analyse the controversy.<sup>11</sup> Warwick's *Masters of Theory: Cambridge and the Rise of Mathematical Physics* argues that theoretical mathematical sciences have been neglected sociologically because of the orthodox view that where experimental science relies on skill, tools and location and is site-specific, theoretical science relies on contemplative isolation and insights of genius and transcends geographical and cultural boundaries. He aims to show the symmetrical nature of analysing theoretical and experimental sciences by focussing on the issue of training within a specific educational location which affected the way in which mathematics and physics grew and took shape in late nineteenth century Cambridge.<sup>12</sup> Constructivist historiography generally keeps a tight focus on the science but uses discipline, training, public, private and scientific space such as the laboratory, classroom and societies and authority as categories of analysis.

## Closure

Scientific controversy can achieve closure in a number of different ways depending on the methodological approach. Beauchamp identifies five methods of closure: sound argument, consensus, procedural, natural death and negotiation. MacMullin identifies three: resolution (epistemic), closure (nonepistemic factors) and abandonment. Both agree that controversies are subject to external as well as internal factors.<sup>13</sup> The positivist approach requires the establishment of scientific truth by confirmation of facts as the criteria for closing controversies whereas the constructivist

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<sup>11</sup> Shapin and Schaffer (1985): 7.

<sup>12</sup> Warwick (2003):11-16.

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approach generally requires consensus on top of confirmation of facts, and places strong emphasis on social factors including decision making as opposed to relying solely on scientific truth for closure. Collins' emphasis is on the negotiations inherent within the scientific social network, especially the core set, because closure cannot be achieved solely by appealing to data due to the difficulty of experimental replication.<sup>14</sup> Many studies use an integrated approach rather than choosing one analytical method as the positivist approach alone is inadequate once you look at social explanations.<sup>15</sup> Even within the constructivist approach there is also a lack of consensus as to which school of thought best portrays the construction of scientific knowledge and one must choose the approach which will draw out the most information without sacrificing the scientific integrity of the study.

In this thesis I confronted the problem by discussing the background to the white dwarf affair, starting with the birth of theoretical astrophysics, the Eddington–Jeans–Milne controversies, the discovery of the limiting mass, the Chandrasekhar–Eddington controversy and then turning to other areas of research which may have influenced Eddington's views. This led me to examine Eddington's contribution to general relativity, relativistic cosmology, Dirac's relativistic equation of the electron and the fine structure constant. I also examined the role of religion in thinking about the creation and the end of the universe. The main focus of my investigation is Eddington's views about singularities. That, I believe, is the crux of the problem. This, in my opinion, is what informed his stance in his controversy with Chandrasekhar, and contributed to his rejection of relativistic degeneracy.

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<sup>13</sup> Engelhardt and Caplan (eds) (1987): 5-7.

<sup>14</sup> Collins (1985): 143.

<sup>15</sup> Martin and Richards (1995): 525.

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As we will see, the thesis will cover the Chandrasekhar-Eddington controversy in technical detail, but in order to understand more fully the reasons behind it, we will need to confront some of the analytical issues outlined above, especially in the case of Eddington whose role in this controversy has not been critically examined in detail before. In attempting to create a more balanced picture, it is important to question which historiographical approach (or combination of approaches) would be most effective in analysing this controversy. Using just one may create a skewed and incomplete picture. In the case of the Chandrasekhar-Eddington controversy, I will focus on the technical aspect questioning why Eddington opposed relativistic degeneracy and the limiting mass of white dwarfs because Eddington's arguments were in themselves scientific. In order to understand *why* he took this position, as the reasons are manifold, I will approach the issue from a constructivist perspective.

## The Controversy

I do not know whether I shall escape from this meeting alive, but the point of my paper is that there is no such thing as relativistic degeneracy!<sup>16</sup>

With this explosive remark Eddington began one of the most interesting and puzzling controversies in the history of modern astronomy and astrophysics. Interesting because it involved stellar constitution and evolution over which there had been frequent witty, and sometimes acerbic, debates between the great British astrophysicists of the early twentieth century, Arthur Stanley Eddington, James Hopwood Jeans and Edward Arthur Milne from the mid-1920s, and puzzling because one would have expected Eddington, who had been the champion of relativity, to support the idea of a new theory

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<sup>16</sup> Eddington (1935a): 38.



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incorporating Einstein's relativity. The arguments and explanations which stemmed from the controversy are not as straightforward as they first appear.

The controversy arose over the theory of the limiting mass for white dwarf stars and the validity of relativistic degeneracy. Subrahmanyan Chandrasekhar first discovered the existence of a mass limit for stars approaching the end of their evolution on the boat journey from India to England in 1930 to start a doctoral degree at the University of Cambridge under the supervision of Ralph Howard Fowler, a physicist and expert in statistical mechanics. As neither Fowler nor Milne (to whom Fowler had shown Chandrasekhar's work) showed any interest in the theory, Chandrasekhar concentrated his efforts on other areas in astrophysics and completed his doctorate in 1933. He became a Fellow of Trinity College later that year and returned to his earlier work to calculate the exact solutions to the relativistic degeneracy formula he had discovered in 1930. With encouragement from Eddington and Milne, he had hoped to announce his discovery at the Royal Astronomical Society meeting in January 1935, and in doing so, resolve the conflict between Eddington and Milne on the internal constitution of stars, in Eddington's favour. Chandrasekhar was in daily contact with Eddington during the four months in which the exact theory of limiting mass was being constructed.

As Chandrasekhar concluded his talk, Eddington stood up and delivered his paper on 'Relativistic Degeneracy.' To Chandrasekhar's astonishment, Eddington fiercely denied the existence of a limiting mass, and claimed that the relativistic degeneracy formula which Chandrasekhar had used

is based on a combination of relativity mechanics and non-relativity quantum theory, and I do not regard the offspring of such a union as born in lawful wedlock.<sup>17</sup>

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<sup>17</sup> Eddington (1935a): 38.

Making further witty but biting comments, Eddington managed to reduce the audience to laughter resulting in Chandrasekhar's theory losing its moment of revelation and conviction. Eddington had convinced the audience that Chandrasekhar's theory was based on shaky conceptual foundations and was therefore inherently flawed.

This was not a bright beginning for a fledgling researcher. Chandrasekhar had been opposed by the very man he had tried to support, whom he revered and with whom he had formed a friendship. And throughout the months before the meeting, although they had met together to discuss Chandrasekhar's work, Eddington had not once voiced his opposition to the theory. And above all, Eddington was not a man whose opinions other scientists would, or could, lightly brush aside.

## **Background to the Controversy: Relativity and White Dwarf Stars**

By the time Eddington began his work on astrophysics, he was already one of the most famous scientists in Britain as a result of the prominent role he played in the 1919 eclipse expedition to confirm one of the predictions of Einstein's theory of general relativity. The coverage in the newspapers, magazines and literature was tremendous and was boosted by the publication of his popular accounts of the theory of relativity. Eddington was a name that was on everyone's lips.<sup>18</sup> He was the first person to hear about, introduce and expose the theory of relativity in Britain in a period of non-communication and strain between Germany and other European countries during the First World War. He was probably the only other person to fully study and comprehend

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<sup>18</sup> Eddington even appeared in Dorothy L. Sayer's detective story, 'Absolutely Elsewhere' in 1934 in which the remark, 'For Heaven's sake, don't go all Eddington' is hurled at Lord Peter Wimsey. Sayers, D.L. (1934), 'Absolutely Elsewhere', *Strand*, February 1934: 185 in Price (2004): 93. Sayers is one of the grand dames of the golden age of detective fiction in the early twentieth century, ranking alongside Agatha Christie and Ngaio Marsh.

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the theory, apart from Einstein himself, Karl Schwarzschild in Germany and Willem de Sitter in Holland, who had communicated Einstein's papers to Eddington who was then secretary of the RAS. And in Britain, he was the sole authority and champion of general relativity. Eddington instinctively believed in the truth of Einstein's theory and set about to ensure that it became fully accepted within the British scientific community.<sup>19</sup> By the time Eddington began to work on stellar structure, he had written several articles and books including the popular *Space, Time and Gravitation* and the more technical *Mathematical Theory of Relativity* which instantly became classical texts amongst scientists and the public and was considered the British expert on general relativity.

The mystery of white dwarf stars had risen in prominence with the publication of Eddington's *Internal Constitution of the Stars* in 1926. White dwarfs are small planet-like remnants of stars, such as our Sun, at the end of the evolutionary scale. They have exhausted their thermal energy supply and are slowly radiating the last of their energy. They are very compact, faint and extremely dense, the most famous example being the companion of Sirius, the brightest star in our sky. Although its existence had been predicted almost thirty years earlier, Sirius B was first observed in 1862, followed by the calculation of its position relative to Sirius. Its mass was comparable to that of the Sun, but was almost 6.45 magnitudes fainter.<sup>20</sup> In 1915, Walter Sydney Adams, a spectroscopist at the Mount Wilson Observatory in the United States obtained a photographic spectra of Sirius B and calculated its radius to be three times that of the

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<sup>19</sup> Earman and Glymour (1980); Chandrasekhar (1987): 110-143; Collins and Pinch (1999): 43-55; Sponsel (2002); Stanley (2003); Warwick (2003): 443-500.

<sup>20</sup> Milne (1932*b*): 5. The magnitude scale for stars is a logarithmic scale such that a first-magnitude star is 100 times brighter than a sixth-magnitude star: the brighter the star, the smaller the magnitude.

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Earth's radius, giving it a density of almost  $68,000\text{g.cm}^{-3}$ . This was a startling discovery because such a high density had not been encountered before.<sup>21</sup>

In 1924, Eddington showed that although this figure was exceedingly high, it was not absurd. He argued that provided a star's temperature is sufficiently high, electrons will be stripped off atoms, and matter ionised to nuclei and free electrons. This complete ionisation will allow close packing of nuclei and hence high densities. The electron gas will remain a perfect gas until maximum density has been achieved, after which deviations from the ideal gas law will occur rapidly.<sup>22</sup>

Eddington suggested an observational test utilising the concept of gravitational redshift from the theory of general relativity to support the density calculations. The high density of Sirius B will mean that its surface gravitation will be very strong compared with terrestrial values and with those from its companion star, Sirius A. Spectral lines from Sirius B will be shifted strongly towards the red by a value which Eddington calculated to be approximately  $20\text{ km.s}^{-1}$ . The following year, Adams observed a displacement of  $19\text{ km.s}^{-1}$ , and the matter was settled. Much to Eddington's delight, this triumph also confirmed Einstein's third prediction in his theory of general relativity.<sup>23</sup>

In the later chapters of *Internal Constitution of the Stars*, Eddington discusses the problem of white dwarfs which remained unfathomable as their origin and constitution were still unknown. At the end of his exposition on white dwarfs, Eddington draws attention to the problem of contraction and expansion which dominate

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<sup>21</sup> The density of water is  $1\text{gcm}^{-3}$ .

<sup>22</sup> Eddington (1926/1988): 165-167.

<sup>23</sup> A redshift is observed when spectral lines are displaced due to the influence of a strong gravitational field which distorts and changes the wavelength of light and other radiation. Einstein's third prediction from his theory of general relativity predicted the bending of light from a strong gravitational field. In this instance, starlight is bent by the sun's gravitational field, and thus the observed position of the stars near the vicinity of the sun will be displaced from their true positions.

the latter stages of stellar evolution. ‘So far as we know,’ he wrote, ‘the close packing of matter is only possible so long as the temperature is great enough to ionise the material.’ As its stellar energy source is depleted, a star would radiate the last remnants of its thermal energy. This would result in a decrease in the thermal pressure which would keep the star balanced against its internal gravitational pressure and the star would commence to contract. But once it nears the complete exhaustion of its energy supply, the star would need to cool down. In order to cool down, however, it would need to expand and work against its gravitational pressure. This would require energy which is no longer available to the star. Thus in Eddington’s view, the star would be in a perpetual state where it needs to cool down, *but does not have enough energy to do so*. He continued, ‘it would seem that the star will be in an awkward predicament when its supply of sub-atomic energy ultimately fails. Imagine a body continually losing heat but with insufficient energy to grow cold!’<sup>24</sup> This was Eddington’s paradox.

In December, a few months after the publication of Eddington’s book, Fowler, an expert on statistical mechanics, published a short paper on ‘Dense Matter’, in the *Monthly Notices of the Royal Astronomical Society*. He was one of the very few people in Cambridge who were interested in the new quantum mechanics being developed by Niels Bohr, Werner Heisenberg and Erwin Schrödinger, and was also Paul Adrien Maurice Dirac’s PhD supervisor. Applying the statistics of Enrico Fermi and Dirac to extremely dense polytropes, Fowler found a solution to Eddington’s paradox.<sup>25</sup> He demonstrated that at such high densities, the stellar gas will be in a degenerate state. This means the following: matter in white dwarfs is in a gaseous state even at high density due to the extreme temperatures involved leading to complete ionisation. At

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<sup>24</sup> Eddington (1926/1988): 172.

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even higher densities, the electrons are squeezed closer together. The Pauli Exclusion Principle requires each electron to be allocated a quantum cell the size of their wavelength. No two electrons in the same state can occupy the same cell. As the density increases, the electrons are squeezed into smaller and smaller cells until the lowest energy levels of the gas are filled. Consequently the cells will begin to exert a pressure countering any further contraction of the gas. This electron degeneracy pressure takes over once the stellar energy source is used up and thermal pressure decreases, thereby balancing the star against gravitational contraction.<sup>26</sup>

The star can now cool down without expanding because once the degenerate state has been reached, 'the temperature then ceases to have any meaning, for the star is strictly analogous to one gigantic molecule in its lowest quantum state' writes Fowler, and stability is achieved.<sup>27</sup> The degenerate electrons in the gas will then effectively be at zero temperature while still possessing high energy.

Eddington welcomed Fowler's contribution which solved his problem. Although there are no direct indications, it is possible Eddington may not have been completely at ease with the introduction of quantum mechanics into his theory. After 1935, Eddington began to attack Fowler's efforts and his utilisation of quantum mechanics and electron degeneracy in order to solve the problem of white dwarfs.<sup>28</sup> Yet in 1926 quantum mechanics successfully solved the paradox without greatly changing Eddington's theory, allowing white dwarfs to achieve a peaceful end. This new view of white dwarf stars put quantum mechanics firmly into the theory of stellar structure and evolution. Electron degeneracy, a central theme in quantum mechanics, was rapidly becoming a central

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<sup>25</sup> The Fermi-Dirac statistics describe the behaviour of gases, such as electrons, that obey the Pauli exclusion principle which states that no two identical electrons can occupy the same quantum cell.

<sup>26</sup> Thorne (1994): 146.

<sup>27</sup> Fowler (1926): 115.



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theme in describing the latter stages of stellar evolution. Despite the incomplete state of white dwarf research, especially the problems concerning energy depletion and stability, the explanation for their high densities seems to have been solved successfully between Eddington and Fowler.

Three years later, in 1929, Edmund Clifton Stoner, a physicist at the University of Leeds specialising in magnetism, turned his attention to astrophysics and dense matter after reading about Jeans' theory of liquid stars. Stoner published a paper in the *Philosophical Magazine* entitled 'The Limiting Density in White Dwarf Stars' in order to see 'whether there is a limit to the electron "congestion"... under the gravitational conditions in stars.'<sup>29</sup> He draws attention to the idea that when compression occurs in an already dense gas, the volume of the cell occupied by a single electron will decrease in size. As the size of the cell depends on the electron's wavelength, this decrease in volume necessitates a decrease in wavelength, thereby increasing the velocity or kinetic energy of the electron. Due to the law of conservation of energy, this kinetic energy must be converted from another form, in this case the gravitational potential energy when the star contracts. Thus the limiting density is reached when gravitational contraction can no longer provide the energy to convert into kinetic energy, thereby preventing the star from further contraction.<sup>30</sup> As the star approaches its end, it will tend towards a limiting mean density due to decreasing gravitational energy.

Wilhelm Anderson of Tartu University in Estonia was working on the same problem simultaneously and pointed out to Stoner that at such high densities, the electrons will be moving at extremely high velocities, and relativistic corrections must be included in the calculations of limiting mass and density. Stoner published a second

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<sup>28</sup> Wali (1990): 141.

<sup>29</sup> Stoner (1929): 65.

paper on the subject a year later in which he incorporated Anderson's relativistic corrections with further refined calculations. Stoner concludes by stating that 'for spheres of increasing mass the limiting density varies at first as the square of the mass, and then more rapidly, there being a limiting mass ( $2.19 \times 10^{33}$ ) above which the gravitational kinetic equilibrium considered will not occur.'<sup>31</sup>

Stoner and Anderson were the first to attempt any calculations of the limiting density and mass in white dwarf stars. Their introduction of relativistic degeneracy into astrophysics hardly caused a stir, and their method was accepted with the resulting equation being named the Stoner-Anderson formula. One of the reasons for this quiet reception was, that despite its novelty, it was a method of calculation. They did not draw any precise conclusions about the limiting density or mass, and certainly gave no explanations regarding the consequences to white dwarfs.

Chandrasekhar began his work on the problem of white dwarfs on his journey to England in 1930. He used Eddington's *Internal Constitution of the Stars* and Fowler's paper 'Dense Matter' as a basis for a more thorough analysis of the problem. Amongst his realisations was that at such high densities, the electrons will be moving at speeds close to the speed of light, and that relativistic corrections were vital to the calculations. Although approximate, his calculations produced a limiting mass of  $1.44M_{\odot}$  (solar masses).

Chandrasekhar worked independently of Stoner and Anderson, and only came to know of their work after reaching Cambridge.<sup>32</sup> Unlike Stoner, however, Chandrasekhar

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<sup>30</sup> Stoner (1929): 65-66.

<sup>31</sup> Stoner (1930): 963.

<sup>32</sup> While in India, Chandrasekhar only had access to the *Monthly Notices*. As Eddington states in a letter to Stoner, the majority of astronomers were more likely to read the *Monthly Notices* rather than the *Philosophical Magazine*, in which Stoner had published his astrophysical papers. Letter of 28 February 1932 (Eddington to Stoner), MS333/164, Stoner Archive.

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approached the problem of the limiting mass by using polytropic models in the Eddington tradition, whereas Stoner's papers relied heavily on Jeans' liquid stars.<sup>33</sup> The conceptual foundation of their papers was completely different.

Throughout the controversy of the limiting mass run strands of earlier controversies between Eddington, Jeans and Milne. Since 1924, Eddington and Jeans had been arguing over the constitution of the stars, whether they were polytropic, completely gaseous throughout and obeyed the ideal gas law, or liquid, where they deviated from the ideal gas law. Milne entered the arena with his Bakerian lecture in 1929, questioning Eddington's method of attacking the problem of stellar constitution and evolution, and introduced another candidate for stellar structure: stars with degenerate cores but ideal gas envelopes. Jeans responded to Milne's entry by stating that there were only two possible stellar structures, polytropic or liquid, further aggravating the situation.

This was the background into which Chandrasekhar entered with his exact theory of white dwarf stars, and he promptly found himself at the centre of the earlier controversies, trying carefully not to take sides.

## **Aftermath of the controversy**

From January 1935 until the death of Eddington in 1944, the controversy raged on with Eddington becoming increasingly more scathing in his remarks. Chandrasekhar himself decided to move on and switched research fields in 1939 after publishing his work on white dwarfs in monograph form as *An Introduction to the Study of Stellar Structure*.

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<sup>33</sup> Chandrasekhar (1931a): 592-596; (1931b): 81-81. Both papers can be found Chandrasekhar (1989).

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Towards the end of 1936 Chandrasekhar moved to Yerkes Observatory to join the Department of Astronomy at the University of Chicago. Until then he had tirelessly argued with Eddington and had corresponded extensively with colleagues such as Dirac, William Hunter McCrea and Leon Rosenfeld in order to gather support and to construct credible arguments to convince Eddington of the existence of relativistic degeneracy. Yet although Niels Bohr and Wolfgang Pauli agreed with Chandrasekhar's analysis and could make no sense of Eddington's, they were reluctant to publicly pronounce on the subject. No one was willing to challenge Eddington's authority.

Chandrasekhar did not pursue the controversy with Eddington apart from on two occasions. The first was at the International Astronomical Union meeting in Paris in 1935, where the conference was chaired by the American astronomer, Henry Norris Russell. To Chandrasekhar's dismay and anger, he was not allowed to reply to Eddington's criticisms.<sup>34</sup> The second occasion, also in Paris, was at the International Conference on Novae and White Dwarfs in 1939. Eddington was present and spoke vehemently against relativistic degeneracy. There ensued a fiery debate between Chandrasekhar and Eddington although neither gave way.<sup>35</sup> Eddington continued to speak against the theory and wrote several articles to this effect until his death in 1944.

## The Thesis

I will attempt to discuss the white dwarf controversy by describing the events leading up to it starting from the mid-1920s when stellar structure and evolution became a major field in which astrophysicists began to work. I will then analyse the controversy and the arguments put forward by Chandrasekhar, Eddington, Fowler and Stoner,

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<sup>34</sup> Wali (1990): 133-134. Chandrasekhar (1977), OHA, NBL.

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discussing the aftermath and the stances taken by various astronomers who were connected to the prominent characters. I will conclude by discussing the possible reasons behind the inevitability of the controversy and the behaviour of Eddington in his opposition to Chandrasekhar's theory. The main questions I will address are as follows:

- Why did the controversy occur?
- Was it inevitable?
- What were Eddington's reasons?
- Were Eddington's arguments as straightforward and purely scientific as they seemed?

Throughout the thesis I will try to show the effect the controversy had on the scientists as well as the influence of the scientists themselves on the course of the controversy and its aftermath. The role of astrophysics and its place in astronomy and the RAS will also be examined through the study of the journals and the scientific correspondence. The structure of the scientific community and that of Cambridge may also throw some light on the events themselves.

The main body of the thesis will be divided into five chapters excluding the introduction, conclusion, appendices and bibliography in the following way.

Chapter one sets the stage by focussing on research undertaken in the early period of stellar astrophysics and white dwarfs. This will cover the period roughly between 1924 and 1931 during which time Eddington was engaged in controversies with Jeans and Milne over stellar structure and methods of investigations. This was the period when stellar astrophysics came into its own with dynamic figures such as Eddington, Jeans and Milne producing a stream of first rate, exciting work which was

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<sup>35</sup> Shaler (1941).

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published one after the other in the *Monthly Notices*, *Nature*, *Observatory* and the *Philosophical Magazine*. Eddington and Jeans both published scientific monographs in the field as well as several popular works on astronomy and cosmology for the general public. There was fierce competition both in their scientific and public lives, ranging from the debates at the monthly RAS meetings to the monthly and weekly journal publications. These debates emphasise the tightness of the scientific community regarding their research and the rate at which information is exchanged. They also show the working friendships which form in such a small community and the way in which they fluctuate according to the reception of various research results which are announced. The theme of friendship and work will run throughout the thesis and can perhaps be used in explaining part of the controversy and the effect it had on Chandrasekhar, Eddington and Milne.

Chapter two will concentrate on the early work which was done on white dwarfs by Eddington, Fowler and Milne as well as their students. From 1929 onwards, it was Milne who encouraged research in this area, often engaging his research students at Oxford. It was also at this time that Stoner and Anderson made their forays into astrophysics, shortly followed by Chandrasekhar and later, Lev Landau, the Russian physicist. I will describe the research conducted on white dwarfs and compare the different methods and ways in which Stoner, Chandrasekhar and others may have come close to finding the limiting mass.

Chapter three will focus on the controversy itself, what transpired at the January meeting in 1935, and the various arguments which unravelled, focusing on Eddington's talk and arguments. I will also discuss the various versions of the controversy which have been published.



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Chapter four will follow the aftermath of the controversy. Chandrasekhar wrote several letters to his colleagues asking and urging them for advice and help in convincing Eddington to change his views. The bulk of his correspondence on the controversy and Eddington's arguments was with Leon Rosenfeld who was at Copenhagen, followed by William McCrea and Dirac regarding points on quantum mechanics which Eddington had raised in his paper 'Relativistic Degeneracy'. I will discuss the correspondence and the arguments raised by the controversy. The correspondence will be supplemented with accounts of the International Astronomical Union meeting in 1935 in Paris and the International Conference for White Dwarfs and Novae which took place in 1939 where Chandrasekhar and Eddington last confronted each other.

Chapter five will aim to explain the reasons behind Eddington's behaviour and his crusade against relativistic degeneracy and the limiting mass. The reasons range from his prior distaste to the concept of singularities, the inability to accept an unstable and unknown ending to white dwarf stars, his reluctance to accept the combination of general relativity and quantum mechanics which Chandrasekhar and Fowler put forward and his increasing absorption in his cosmology and what later came to be known as his fundamental theory. The different reasons may shed a light on the enigmatic character of Eddington who spent the last ten years of his life working alone on his fundamental theory and fighting relativistic degeneracy. I will bring the various strands together to argue that Eddington's rejection of Chandrasekhar's limiting mass is due to his rejection of singularities which may be explained by his intuition, his cosmological stance and the impact of his fundamental theory.

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The conclusion will bring together the different parts of the story surrounding the controversy including the historiographical approach considered in analysing the controversy and its resolution.

## Sources

My sources for the discussions in my thesis are taken from archival material, primary and secondary literature. I have researched material in several archives, including the Chandrasekhar, Eddington, Milne and Stoner archives as well as interviews with several astronomers in the Oral History Archives at the American Institute of Physics. Eddington had requested that his personal notes be destroyed after his death, and his archive is very sparse compared to those of Chandrasekhar and Milne.

I will discuss the literature and analyse it in comparison to my thesis regarding Eddington's views on singularities to clarify his rejection of relativistic degeneracy. As we shall see, there are only a handful of attempts to discuss the Chandrasekhar-Eddington controversy in detail, although it is mentioned frequently, but briefly, in the secondary literature. The primary literature also only yields a narrow discussion regarding relativistic degeneracy and the limiting mass, although Eddington's views on singularities are more forthcoming.

The primary literature used in this study can be divided into two categories: technical and popular. The technical literature consists of textbooks and astrophysical and mathematical papers published in scientific journals such as *MNRAS*, *Observatory*, *Astrophysical Journal*, *Philosophical Transactions of the Royal Society* and the *Philosophical Magazine* amongst others and are written for other astronomers, physicists and mathematicians. These were used to describe and analyse the various

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astrophysical controversies in which Eddington was involved. Even though they are technical, one can still get a powerful sense of the controversies through the colourful language used in the introductions and conclusions of the papers. The remarks are caustic, but witty, especially in Eddington's case, and one can see the verbal relay between the scientists as they tried to parry and defend their ideas against criticism from one another. The number of papers published in the journals within the space of two years for each controversy, especially in *MNRAS* and *Observatory*, is astonishing. The intellectual output by the astrophysicists is overwhelming, and we can see that most of the papers, although complete, show how their research evolves under the close scrutiny of their peers.

The popular literature is also written by the main characters in this study but is aimed at a much wider audience: the public. These can be roughly divided into two sub-categories: expositions of new scientific theories and discoveries and biographical memoirs written by, and about, the astrophysicists. The first consist mainly of popular expositions of the theories of astronomy and astrophysics, general relativity, quantum mechanics, cosmology and epistemology which were current at the time. The majority of these popular books were written by Eddington and Jeans. The second contains biographical and anecdotal memoirs of Chandrasekhar, Eddington, Fowler, Jeans and Milne, but also include some peripheral characters such as Dirac who had a great impact on Chandrasekhar. Apart from the obituaries, which were written by their peers - Milne had written both Fowler's and Jeans' - a substantial amount of what we know about the astrophysicists and their work come from the various articles written by Chandrasekhar over the years. These articles are thorough and rigorous dealing with both the social and technical aspects of the astrophysicist lives and careers, especially that of Eddington and

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Milne.<sup>36</sup> Most of the subsequent secondary literature dealing with Chandrasekhar, Eddington and Milne, especially on the limiting mass of white dwarfs, use Chandrasekhar's articles and archival material as their source – in fact most of them seem identical, except for variations in detail and length. It is also interesting to see that all of Chandrasekhar's articles, which were written over a period of twenty-odd years, are consistent regarding his views on Eddington, Milne and the controversy, and never wavers from the original story told by Chandrasekhar. This would indicate that Chandrasekhar deliberately made them consistent. After all, the overwhelming majority of the secondary literature published about him had his controversy with Eddington as the central theme. Chandrasekhar makes it clear, in his writings, that his friendship with Eddington was not affected by the controversy in any way. However, archival material and interviews with his colleagues show otherwise. Many have said that he remained very bitter about the whole affair, and this also comes across sympathetically towards Chandrasekhar in the secondary literature.

The secondary literature can be divided into three categories: biographical focussing on the life and science of the astrophysicists; analytical where the scientist and his research are analysed within a historical, scientific, social or philosophical context; and scientific focussing on general scientific topics such as astronomy and astrophysics, general relativity, quantum mechanics and white dwarfs.

Almost all the material on Chandrasekhar, especially the obituaries, makes a point of mentioning the controversy with Eddington over the limiting mass. This is the same for all material on white dwarf stars. It is not the same, however, for Eddington, who was involved in several controversies throughout his career. The secondary literature is diverse, ranging from his research in stellar structure, cosmology,

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<sup>36</sup> Chandrasekhar (1976a); (1987); (1993).

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fundamental theory, philosophy and religion. We can almost say that a considerable part of Chandrasekhar's fame may have stemmed from the notoriety of his clash against Eddington.

Of the biographical literature, the two main book-length sources are Alice Vibert Douglas' biography of Eddington and Kameshwar C. Wali's biography of Chandrasekhar.<sup>37</sup> Wali has written one preliminary article about the Chandrasekhar-Eddington controversy for *Physics Today* from which his biography of Chandrasekhar evolved.<sup>38</sup> These two biographies of Chandrasekhar and Eddington are the most comprehensive and thorough biographies of the two scientists which are currently available and both draw on archival material and interviews with colleagues and friends. Both volumes are written as popular biographies of the two astrophysicists and are not aimed at historians, thus lacking thorough referencing and critical discussions of Eddington and Chandrasekhar within their social, scientific and philosophical contexts. As narratives, they give a colourful description of their subjects and manage to encompass all the major events in their lives. However, both biographies are heavily biased towards their subjects and one can clearly feel the awe in which the authors regarded them. Douglas, an astrophysicist from Queen's University, Toronto, was Eddington's research student in the early 1920s, and Wali is a physicist from Syracuse University who had followed Chandrasekhar's career closely, becoming his biographer and friend. Regarding Douglas' biography, Chandrasekhar has this to say, 'Miss Douglas'[s] biography of Eddington is full of mistakes; and she misuses some of the

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<sup>37</sup> Douglas (1956); Wali (1990).

<sup>38</sup> Wali (1982).

material I had given to Trimble who was to have written the biography but died before he had embarked on the project.)<sup>39</sup>

Regarding the controversy over the limiting mass of white dwarfs, there are only a few pages in Douglas in which she mentions Chandrasekhar, Stoner and Anderson's work on the limiting mass of white dwarfs. She does not comment on the consequence of the limiting mass, only that it brought back the same difficulty, Eddington's paradox, from which Fowler had rescued it five years earlier.<sup>40</sup>

Wali, on the other hand, discusses the controversy in one and a half chapters, and examines the Chandrasekhar-Eddington controversy within the context of the Eddington-Milne controversy which was taking place when Chandrasekhar first arrives at Cambridge.<sup>41</sup> He believes that Chandrasekhar, stumbling into the Eddington-Milne controversy, felt that his work on the limiting mass will settle their differences regarding stellar structure. Wali uses extensive archival material and interviews with Chandrasekhar's colleagues to build a picture of the controversy as it happened, but his work is extremely sympathetic towards Chandrasekhar, portraying him as suffering from a great betrayal by Eddington, one of his heroes. Wali heavily underlines his opinion, shared by Chandrasekhar's widow, Lalitha, that Eddington was unsportsmanlike in keeping his agenda hidden. Wali strongly believes that Chandrasekhar did *not* know that Eddington would disagree with his theory, and that Eddington's paper, 'On 'Relativistic Degeneracy'', which he presented at the January 1935 meeting at the Royal Astronomical Society, came as a severe, and unexpected, shock to him.<sup>42</sup> He discusses

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<sup>39</sup> Reflections and reminiscence: Arthur Stanley Eddington, Chandrasekhar Archive, Addenda Box 77/ folder 5. Charles Trimble was Eddington's closest friend.

<sup>40</sup> Douglas (1956): 160-162.

<sup>41</sup> Wali (1990): 119-146. A discussion of the Eddington-Milne controversy can be found in pages 119-120.

<sup>42</sup> Wali (1990): 123, 126.

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Chandrasekhar attempts to turn the tide of opinion in his favour and to convince Eddington that relativistic degeneracy was a valid concept, but he does not discuss why the controversy occurred and the reasons behind Eddington's refusal to accept relativistic degeneracy, except stating that Chandrasekhar had described Eddington as being extremely confident of himself, not caring what anyone thought, and had succeeded in demolishing Chandrasekhar's work 'by a few flippant remarks'.<sup>43</sup> Wali's biography is essentially about Chandrasekhar, he does not attempt to understand why Eddington behaved in this way, only commenting on the way this controversy has affected Chandrasekhar and the course of research on singularities, which was, according to Chandrasekhar, probably delayed by about thirty years.

William Hunter McCrea, on the hand, has written articles about both Chandrasekhar and Eddington, knew both of them and was also present at the RAS meeting in January 1935. Although he does not focus exclusively on the white dwarf affair, he does mention it, and is almost exclusively the *only* person to insist that Chandrasekhar probably knew about Eddington's reaction to relativistic degeneracy and his speech that sparked the controversy was not completely unexpected. McCrea also insists that neither Chandrasekhar nor Milne harboured any ill-feeling towards Eddington after their controversies. He believed that the scientific controversies were conducted solely within the confines of the RAS and the pages of its journals, and once the astrophysicists left the scientific arena, they also left their quarrels behind. The scientific battles did not extend into their personal friendships. McCrea is, however, silent regarding Eddington's motives behind his rejection of relativistic degeneracy,

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<sup>43</sup> Wali (1990): 127, 144.

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seeing his disagreement with Chandrasekhar as another in a long line of scientific disagreements which Eddington enjoyed.<sup>44</sup>

The majority of the remaining biographical articles about Chandrasekhar are obituaries which were published in the leading scientific journals and newspapers. Many, as I have mentioned earlier, focus on the controversy and Eddington's ill-treatment of Chandrasekhar, portraying Eddington as abusing his authority.

The number of articles and books analysing the controversy in any depth is very small. Apart from Wali's biography of Chandrasekhar, there is only a handful that actually goes to any length in explaining the controversy in terms of Eddington's motives. Werner Israel's paper, 'Dark Stars: An Evolution of an Idea' and Clive Kilmister's, *Eddington's Search for the Fundamental Theory*, are two which try and give a reason why Eddington may have rejected relativistic degeneracy and the limiting mass.<sup>45</sup> Others such as Kip S. Thorne's *Black Hole and Time Warps* and G. Venkataraman's *Chandrasekhar and his Limit* provide good descriptions of the controversy but shy away from making any assessments. They, as the others, use Chandrasekhar's articles and memoirs as the main sources for their study of the controversy.

Although the controversy between Chandrasekhar and Eddington goes down in scientific history as one of the great controversies of the twentieth century, there has been very little historical research on the subject. Eddington himself one of the most famous British scientists of the twentieth century, and who had influenced whole generations of astrophysicists, mathematicians and physicists, remains an enigma. Anecdotes of him abound but we still do not fully understand what drove him to pursue

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<sup>44</sup> McCrea (1991); (1996).

<sup>45</sup> Israel (1987); Kilmister (1994).



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his later research choices and to make his particular stand. Although the controversy is the focal point of this thesis, it is also a springing board into Eddington's mind and the various strands of research which he pursued in the final phase of his career.

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## CHAPTER ONE: Early Astrophysical Controversies

The Chandrasekhar-Eddington controversy was not the first to emerge from the Royal Astronomical Society (RAS) monthly meetings at Burlington House in London. During the early twentieth century, astronomy was rife with controversies ranging from the formation of galaxies to the classification of stars and cosmology. This was to a large extent due to the imperfect data on which physical theories such as quantum physics, atomic physics and the physics of energy generation relied. The turn of the century produced new measuring and observing instruments, new methods of calculation and analysis and new theoretical devices. It also witnessed the birth of a new field: astrophysics.

Observational astrophysics in the late nineteenth century was dominated by Norman Lockyer and Alfred Fowler who were both conducting research in London at the Solar Physics Observatory in South Kensington. They pioneered the study of spectroscopy and applied it to stellar radiation. Their research uncovered the chemical composition of stars from their spectra, which also revealed the presence and magnitude of magnetic fields when the spectral lines were shifted. The spectra indicated as well the surface temperature of stars which is a key to their classification and to the study of their evolution. Since the late nineteenth century, photography and photometry were increasingly utilised in observational astrophysics, especially in the United States, where the majority of large-scale, working telescopes, such as the Hale and the Mount Wilson, were located.<sup>1</sup> Using photography and photometry to study the solar spectrum, Arthur Schuster in Manchester in 1905 and Karl Schwarzschild in Göttingen in 1906 independently worked out the equations of radiative transfer of energy in the solar

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<sup>1</sup> Meadows (1984): 3-15.

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surface culminating in a model of the sun in which the temperature and density of matter increases with depth.<sup>2</sup>

Although observational astrophysics flourished roughly equally in Britain and the United States, theoretical astrophysics, on the other hand, was the intellectual property of British astronomy in the early twentieth century. Led by Arthur Stanley Eddington, James Hopwood Jeans and Edward Arthur Milne, much of the research was conducted and communicated via the Royal Astronomical Society in London and its publications, the *Monthly Notices of the Royal Astronomical Society (MNRAS)* and the *Observatory*. Starting from roughly 1916 onwards both Eddington and Jeans began to contribute to problems in theoretical astrophysics stemming from research on stellar structure such as the period-luminosity of Cepheid variables, the radiative equilibrium of stars and the mass-luminosity relation. The criticisms generated by their work started a wave of disputes between the two astrophysicists beginning with a series of articles and correspondence in the *MNRAS*, the *Observatory* and *Nature*. The quantum physicist Paul Adrien Maurice Dirac, who had just commenced his studies at Cambridge, recalled that all the students were interested in the Eddington-Jeans controversy and avidly read the relay of letters published in *Nature*.<sup>3</sup> They rapidly expanded to public debates at the RAS monthly meetings until 1929 when Milne entered the scene criticising both Eddington's and Jeans' work and proposing a rival theory to describe stellar structure. This increased the ferocity and frequency of subsequent attacks and provided 'intellectual entertainment' for the members of the RAS, many of whom had joined the society expressly to view the increasingly popular spectacle. Milne recalls that the Oxford mathematician Godfrey Harold Hardy joined the RAS 'in order to have the

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<sup>2</sup> North (1994): 476.

<sup>3</sup> Dirac (1963), Oral History Archive, Niels Bohr Library.

privilege of attending these debates, and hearing Eddington and Jeans castigate one another in public. And sure enough, Hardy attended the debate of January 1931, in due course, when the conflict had become triangular.<sup>4</sup> By the early 1930s, these debates had become legendary: many viewers claimed both Eddington and Jeans enjoyed these playful spates using their razor-sharp wits to drive their opponent down. In his article on Eddington, Sir William McCrea, a student of Fowler's and a contemporary of Chandrasekhar, Eddington and Milne, writes,

the resulting debates at monthly meetings of the Royal Astronomical Society during the mid-1920s attracted unprecedented interest. Many of the country's most famous scientists attended - at least one leading mathematician became a Fellow of the Society just to have the right to be there. Eddington and Jeans gave each other no quarter; they could behave in this way - and enjoy doing so - because privately they were on excellent terms.<sup>5</sup>

Whether they were on 'excellent' terms in private, as McCrea believes, is highly debatable. Although controversies at the RAS were conducted along the lines of a gentlemanly code and etiquette, not everyone remained unscathed, especially the younger researchers who were new to the professional arena.<sup>6</sup> This can be seen especially in the reaction of Milne when confronted by both Eddington and Jeans during the Eddington-Milne controversy from 1929-31, about which George Cunliffe McVittie, a former research student of Eddington's, said in an interview,

I've often been told by older members that there had been very acid exchanges, on the subject of stellar structure between Eddington and Milne, Jeans, and so on.<sup>7</sup>

And is echoed by James Gerald Crowther who writes about Eddington,

The 'quiet effrontery' of his literary style, which amused scholars in the same Cambridge intellectual circles, often angered those without,

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<sup>4</sup> Milne (1952): 31. The controversy became triangular with the participation of Milne.

<sup>5</sup> McCrea (1991): 69.

<sup>6</sup> Hetherington, 'E.A. Milne', Chandrasekhar Archive, Box 107/ Folder 7: p.11.

<sup>7</sup> McVittie (1978), Oral History Archive, Niels Bohr Library.

who were earnestly trying to understand the crises in modern life reflected in physics.<sup>8</sup>

I will begin this chapter by giving a brief analysis of the range of topics in the Eddington-Jeans controversy from 1916-28 and continue with the Eddington-Milne controversy on stellar structure from 1929-31. As well as the technical disputes, I will be looking at the effect of the controversies on the personal and professional relationships between the parties involved and the impact of astrophysics within the astronomical community.

## **1.1 The Eddington-Jeans Controversy: 1916 - 1928**

The Eddington-Jeans controversy spanned almost two decades beginning a few years after Eddington left the Royal Observatory at Greenwich and moved to Cambridge to take up his new position as Plumian Professor of Astronomy and Astrophysics at the end of 1912 and Director of the Cambridge Observatory in the following year. This geographical shift coincided with Eddington's intellectual shift in academic interest from observational and practical astronomy to general relativity and theoretical astrophysics.

Born in 1882, Eddington demonstrated a keen interest in numbers as a child and excelled in mathematics at school. He won scholarships to Owens College, the future University of Manchester, where he was taught physics and mathematics by Arthur Schuster and Horace Lamb, and later Trinity College, Cambridge, where he sat for the Mathematical Tripos.

After completing the Mathematical Tripos, Eddington began some mathematical research on the electronic theory of matter as well as conducting experimental work at

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<sup>8</sup> Crowther (1952): 189.

the Cavendish Laboratory for a year before accepting the position as Chief Assistant at the Royal Observatory in Greenwich. From 1906 to 1913, Eddington spent the next seven years of his life at Greenwich, learning how to use the telescopes and other time-measuring devices and to improve his observational skills. Work at the Royal Observatory was concentrated mainly on positional astronomy and the measurement of proper motions and the distribution of the stars.<sup>9</sup> There can be no doubt that Eddington acquired a thorough training in practical astronomy and the methods required for analysis.<sup>10</sup>

But once he became Director of the Cambridge Observatory, Eddington rarely performed any observations there, and turned towards theoretical and mathematical research. The sole exception was the solar eclipse expedition to Principe in November 1919 to verify the gravitational bending of starlight, one of the predictions of Einstein's theory of general relativity.<sup>11</sup>

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<sup>9</sup> The proper motion of a star is the component of its velocity at right angles to the line of sight. Pioneering work on star-streaming was conducted by Jacobus Cornelius Kapteyn at Gröningen to explain the motion of stars. In 1904, Kapteyn put forward his theory of the two star-streams, claiming that, having studied the proper motions of stars in various regions of the sky, he found that instead of the popular opinion that the motion of the stars was completely random, they moved in two general directions relative to the sun. But within each direction, there was random stellar motion. This theory was later superseded by the spiral arm theory put forward by astronomers such as Henry Norris Russell at Harvard who placed the motion of the galactic stars within the context of the general motion of the spiral arms of our galaxy. References can be found in the following papers on stellar motion and distribution by Eddington in the *MNRAS* **67** (1906): 34-63; **68** (1908): 588-605; **74** (1914): 5-16; **75** (1915): 366; **76** (1915): 37-60; Eddington (1914); Chandrasekhar (1987): 97-101.

<sup>10</sup> Eddington familiarised himself with the Cookson Zenith Telescope and initiated an observational programme to determine latitude variations and the constant of aberration. He immersed himself with work on the transit-circle, and trained in meridian work. Observational material taken at Greenwich also provided him with the opportunity to study the formation of the envelopes of the Comet Morehouse. The Cookson Zenith Telescope at Greenwich was a large refracting telescope designed to observe stars as they pass overhead. Latitude variation is the small variation in the latitude of the observing site rising from the motion of the Earth's geographical poles due to the wobble of the Earth's rotational axis. The constant of aberration is the ratio of the Earth's mean velocity to the speed of light and is the maximum amount by which a star can appear to be displaced from its mean position. Due to the motion of the Earth around the Sun, there is a small displacement in the image of a star throughout the solar year.

<sup>11</sup> There are several accounts of the famous solar eclipse expedition in 1919 which verified Einstein's prediction that starlight is bent near a large gravitational field (in this case the Sun's). The official account can be found in the *Philosophical Transactions of the Royal Society A* **220** (1920):291 as well as several articles in the *MNRAS* and in the *Observatory* **43** (1920):33. There are also accounts in Eddington (1920c); Douglas (1956): 38-42; Chandrasekhar (1976) and (1987): 93-143. Earman and Glymour (1980)

Jeans' early academic career was almost identical to that of Eddington's. Born five years earlier in 1877, Jeans came from a strict religious family. Like Eddington, he acquired a deep-seated interest in numbers from a very early age. His intellectual ability earned him a scholarship to Trinity to prepare for the Mathematical Tripos winning the Smith's Prize in 1901 and becoming a Fellow of Trinity College. In 1912, Jeans retired from lecturing at Cambridge after only two years of being the Stokes Lecturer in Applied Mathematics, although he kept a few lecturing appointments in the United States, mainly at Princeton University where he had previously lectured between 1905 and 1909. Jeans' affluence allowed him to lead the life of a country gentlemen but, apart from the monthly meetings at the RAS and the popular lectures he delivered to the public, he did not participate much in academic circles, except via publications, and preferred instead to lead an isolated existence.<sup>12</sup>

In his biographical account of Eddington and Jeans in *Great Scientists of the Twentieth Century*, Crowther comments that when the Plumian Chair for Astronomy and Astrophysics fell vacant following the death of Sir George Airy in 1912, Jeans had also been a candidate for the post but instead, the Chair went to Eddington who was five years his junior. Crowther insists that Jeans had been very upset about being passed over for the post, which had belonged to his former teacher, and this may have caused him to shun the scientific community he had once been a part of.<sup>13</sup>

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throw a critical light on the scientific credibility of the results of the expedition, Stanley (2003) argues for the religious motivation behind the expedition, Sponsel (2002) illustrates the expedition as a study of public relations and propaganda and Collins and Pinch (1993): 43-55 extends Earman and Glymour's account to show how Eddington's manipulation of theory and results could only produce the expected result of Einstein's prediction. There is a cache of letters from Eddington to his mother surviving from his trip to Principe which are deposited in the Wren Library, Trinity College, Cambridge.

<sup>12</sup> Milne (1952): 3-17.

<sup>13</sup> Crowther (1952): 104. This is the only reference to the incident which does not appear in any of the biographical material on Jeans.

The years 1912 to 1913 were significant in the lives and careers of both Eddington and Jeans: it is the starting point when both astronomers began to turn towards astrophysics and develop theories in the new field. It also marks the beginning of their long rivalry in the academic and public spheres.

The controversy between Eddington and Jeans can be divided into roughly five main astrophysical categories spanning from Eddington's work on radiative transfer and equilibrium, Cepheid variability, the source of energy generation in stars, the mass-luminosity relation and stellar structure. These topics occur in a loose chronological order often overlapping and merging, spanning almost two decades from 1916 when Eddington published his first paper on radiative transfer. Eddington's theories were novel and intuitively bold, a trait for which Eddington was to become famous, but they were supported by extremely rigorous mathematical calculations and derivations.

Eddington's former research student and official biographer, Alice Vibert Douglas claims that Jeans' main objections were 'not on the main idea which was obviously of prime importance, but on details of derivation and interpretation.'<sup>14</sup> We shall see whether this is the case, as both men had great mathematical ability and adaptability, Eddington having been Senior Wrangler and Jeans Second Wrangler in the second year examinations of the Cambridge Mathematical Tripos.<sup>15</sup> They were both involved in introducing new physical theories into Britain, both produced reports for the

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<sup>14</sup> Douglas (1956): 61, 74. Chapter 7 of the biography deals with Eddington's contribution to stellar physics and Douglas analyses the various controversies between Eddington and Jeans, quoting from accounts of RAS meetings which are published in *Observatory*. Douglas' biography of Eddington tends to be rather biased towards Eddington. In the early 1920s Douglas was a research student working under Eddington, together with Cecilia Payne-Gaposchkin whose pioneering research in stellar astronomy aided her in becoming the first female Professor of Astronomy at Harvard. For Payne-Gaposchkin's autobiography and other related articles see Haramundanis (1996).

<sup>15</sup> Most of the mathematicians who proceeded to carve out careers in the physical sciences ranked highly in the Mathematical Tripos, yet it was extremely rare for a student to become Senior Wrangler in the second year of the Cambridge Mathematical Tripos. See Milne (1952): 5; Douglas (1956): 11; Warwick (2003): 450.



Physical Society of London: Jeans on quantum mechanics in 1914 and Eddington on general relativity in 1918. Douglas' account of the controversies between Eddington and Jeans are gathered from the accounts published in the *Observatory* and tries to explain the main ideas behind the theories put forward. There is no attempt to interpret the controversies within the social context of the British astronomical community in the 1920s and within the personal rivalry between the two men which extended beyond the *purely* scientific and into the *popular* scientific arena.

If we analyse the points of controversy between the two astrophysicists, we find that Jeans' main objections to Eddington's work can be separated into three main categories:

1. Eddington's theory of radiative equilibrium in which 'radiation pressure = gravity'.
2. The source of energy generation whether it is due to Helmholtz contraction or radioactivity.
3. The mass-luminosity relation.

### **1.1.1 Radiative Equilibrium and Cepheid Variability**

Beginning in 1916, Eddington's first foray into theoretical astrophysics was to construct a theory to explain the periodic luminosity variation of Cepheid variable stars, large gas giants with low density that emit a pulse of light at constant intervals of time. In order to do so, he needed to investigate the following roles which radiation played in a star: the transfer of heat and light through a star and the balance against gravitational pressure to establish stellar equilibrium. He did this by using a model of a gaseous ball or *polytrope* obeying the perfect gas law.

The underlying principles used in constructing stellar models are hydrostatic equilibrium, the perfect gas equation, the different modes of energy transfer (including convection, thermal energy transfer and radiative energy transfer) and sources of stellar energy. These involve parameters such as temperature, mass, density, pressure, luminosity, rate of energy production and the mean molecular mass which are linked together via the equations of state. By specifying boundary conditions, it is possible to see how the parameters vary throughout the interior of the star.<sup>16</sup>

Eddington's theory of polytropes can be broadly defined in the following way. The stellar model he uses is that of a giant gaseous star of uniform density which obeys the perfect gas equation throughout. Thus the four main equations which he utilises in his work are the following:

$$\text{hydrostatic equilibrium} \quad dP/dr = -GM\rho/r^2 \quad (1)$$

$$\text{radiative equilibrium} \quad L = [-64\pi\sigma r^2 T^3]/[3\kappa\rho] (dT/dr) \quad (2)$$

$$\text{perfect gas law} \quad P = nkT \quad (3)$$

$$\text{and polytropic equation} \quad P = \kappa\rho^\gamma \quad (4)$$

where  $P$  is the interior pressure,  $r$  is the radius,  $M$  is the mass,  $G$  is the gravitational constant,  $L$  is the luminosity,  $\sigma$  is the Stefan-Boltzmann constant,  $\kappa$  is the opacity,  $\rho$  is

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<sup>16</sup> Zeilik and Smith (1987): 277.

the density,  $r$  is the radius,  $T$  is the temperature,  $n$  is the number density of particles,  $k$  is the Boltzmann constant and  $\gamma$  is the ratio of specific heats.

Eddington followed in the footsteps of Robert Emden, Jonathan Homer Lane, August Ritter and Karl Schwarzschild in studying the radiative transfer of energy in stellar atmospheres using the polytropic model.<sup>17</sup> But he applied it specifically to the *interior* of stars, being very careful to state that his work only related to the interior of stars and not the outermost layers, and therefore they had no bearing on the interpretation of spectroscopic results.<sup>18</sup> By adopting radiative transfer as the main mode of energy transfer, attention was focused on the increasingly important role of opacity.<sup>19</sup> The main question was not to ask how heat is brought to the surface, but how the heat in the interior of the star can be held back and prevented from leaking. Considering the amount of energy being generated in a star, the observed luminosity or rate of outward flow of radiation was not very high, and Eddington describes a star as a ‘storehouse of heat’.<sup>20</sup> In fact, each layer in the interior of a star absorbed the radiation flowing through it and extended the time which the radiation took to flow to the stellar surface. This idea focused attention on opacity, and Eddington began to study how radiation travelled through stellar material and how the gaseous layers absorbed and scattered the radiation.

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<sup>17</sup> DeVorkin (1984): 91; North (1994): 475. Jonathan Homer Lane’s work on stars as perfect gas spheres in 1869 was ‘the first to construct a theoretical model of the Sun wherein one could determine the temperature, density and pressure found at any point within the solar interior.’ If a star was to decrease in radius through gravitational contraction, as long as perfect gas conditions were maintained, there would be a homologous rise in temperature, including surface temperature. If a star cannot maintain the perfect gas state, further contraction can only occur through cooling. In the 1870s, August Ritter, Professor of Mechanics at the Polytechnical School in Aachen, independently found the same solution to the problem as Lane, creating a theory of stellar evolution based on convective equilibrium where there is an initial heating phase when the star is a perfect gas before cooling begins. Before this, stars were thought to just cool down. Ritter’s papers were published in the *Astrophysical Journal* in the 1890s. In 1907, Robert Emden, assistant professor of physics and meteorology at the Technische Hochschule in Munich published *Gaskugeln*, the first comprehensive work analysing Lane and Ritter’s work on polytropes.

<sup>18</sup> Eddington (1916): 16.

<sup>19</sup> The opacity of a star is defined as the amount of radiative energy absorbed and describes the degree of non-transparency in a gas.

<sup>20</sup> Eddington (1920b): 343.

He found that the radiative pressure was directly related to the opacity of the stellar material. As radiation passes through the layers of gas, the momentum of the radiation is constantly changing due to the absorption of energy by the gaseous molecules. This will decrease the amount of radiation which leaves the material and aid the balance against the gravitational pressure inwards.

Until Eddington's research on stellar equilibrium, it was assumed that the radiation and energy within a star were distributed by convection.<sup>21</sup> Therefore thermal pressure, via convection, was considered the sole balancing force against gravity in maintaining stellar equilibrium. Eddington, on the other hand, distinguished thermal (material) pressure from radiative (aetherial) pressure and insisted that radiative transfer was the main mode of energy transfer within a star. In doing so, he elevated the role of radiation pressure in maintaining stellar equilibrium.<sup>22</sup> A sequence of papers on radiative transfer was published in the *Monthly Notices*, between 1916 and 1919 in which Eddington, who was working mainly with polytropic models of giant stars of low density, concluded that

a rarefied gaseous star adjusts itself into a state of equilibrium such that the radiation pressure very approximately balances gravity at interior points.<sup>23</sup>

Thus in the first phase of his astrophysical work, Eddington's contribution was to draw out the importance of radiation, rather than convection, as the primary mode of energy transfer, and to establish the concept of radiative equilibrium.

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<sup>21</sup> Lane, Emden and Schwarzschild had discussed earlier the important role of radiation pressure with respect to thermal pressure in the stellar atmosphere. Eddington was the first to extend their work into the interior of the star. Eddington gives a list of references in Appendix II of Eddington (1926/1988): 397. Convection is defined as the transport of heat via the movement of the heated substance.

<sup>22</sup> Eddington (1916), (1917a), (1917e), (1918). Thermal energy was considered to be the kinetic energy of the atoms in the stellar gas due to the motion of the atoms, whereas radiation was electromagnetic or light waves.

<sup>23</sup> Eddington (1916): 16.

Although Eddington was one of the first scientists who studied Einstein's general relativity to discard the aether as a medium through which electromagnetic waves travelled, he frequently uses the terms 'aether' and 'aetherial waves' to describe the space surrounding the earth and light waves in his work in the Maxwellian tradition of Cambridge physics. This should be taken as a descriptive tool and not a lingering adherence to the physical concept before Einstein's theory of 1915. Eddington's descriptive method of writing is mainly analogical, comparing astrophysical and relativistic concepts to ordinary, everyday phenomena or scientifically familiar concepts.

The reason for Jeans' first objection to Eddington's assertion that 'radiation pressure = gravity' is that the dimensions are wrong. The second is that Eddington uses the relation not as a numerical approximation but a fundamental physical equation. To this Eddington replies that Jeans is quibbling and that the expression Eddington had used, 'radiation pressure = gravity', was not to be taken as a literal statement but that it was an analogy, a popular form of expressing an idea on which scientists often relied. What Eddington was trying to show was that for low density gas giants, or polytropes, a larger proportion of the pressure balancing gravitational contraction was in fact due to radiation pressure.<sup>24</sup> Jeans points out that radiation pressure must depend on the mass: as the stellar mass increases, the ratio of radiation pressure with gas (thermal) pressure increases. Eddington, on the contrary, has shown in his papers that the emission of radiation from a star which obeys the perfect gas law will *not* vary as it contracts. As long as the opacity remains constant the balance between radiation pressure and gravity remains constant. But according to Jeans, the emission is only constant if the energy is from gravitational contraction.

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<sup>24</sup> Eddington (1917c); Jeans (1917a); Jeans (1919): 319.

Another objection which Jeans voices over radiative equilibrium is the hypothesis that a star can adjust its radius and surface temperature to establish radiative equilibrium at any time. Eddington argues that if a star did not do this, it should expand if the radiation flowing out is greater than gravity. But as we do not see this happening, Eddington assumes that the star does not generate energy at a given rate, but modifies according to whether it is expanding or contracting. Jeans remained unimpressed and concludes that

the old fashioned sphere of gas in which radiation was left entirely out of account still provides a remarkably good model of a star.<sup>25</sup>

Jeans is certainly not happy with the idea of a radioactive source of energy in stars, and prefers to investigate stellar models where energy is formed via Helmholtz contraction arguing that

the rate of emission of energy, being also the rate of generation of energy in the star's interior must depend on the ultimate source of this energy. ... Hence a preliminary to any attack on the general problem must be a decision as to the source of energy. Many investigations have shown that the generation of energy arising from radioactivity in a star can be at most only a small fraction of that produced by gravitational contraction. ... It accordingly appears that a fair approximation to actual conditions will be obtained by regarding the star as a mass of gas contracting under its own gravitation, and having no sources of energy except those of gravitational contraction.<sup>26</sup>

To this Eddington replies,

If the contraction theory were proposed today as a novel hypothesis I do not think it would stand the smallest chance of acceptance. ... Only the inertia of tradition keeps the contraction hypothesis alive - or, rather, not alive, but an unburied corpse.<sup>27</sup>

The source of stellar energy was one of the main criticisms which Eddington encountered, especially from Jeans, during the course of his investigations. The

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<sup>25</sup> Eddington (1925*b*) p.404; Jeans (1925*c*): 797.

<sup>26</sup> Jeans (1917*c*): 36.

<sup>27</sup> Eddington (1920*a*): 18.

established theory in the early twentieth century for the source of stellar energy was Helmholtz (or gravitational) contraction: a star would generate energy by contracting slowly and the energy produced would flow out of the star as radiation. Although the generation of energy was not a main feature in Eddington's theories, nevertheless he did contribute in speculating on its source. Eddington regarded the contraction theory as producing too little energy to account for the long life-span of any star and argued that the most plausible source was sub-atomic matter or radioactive matter because

in an actual star the stream of energy flowing outwards is supplied by slow changes occurring within the star. The simplest theory results if we suppose that the energy is produced by radioactive processes.<sup>28</sup>

His early papers show that Eddington did not restrict energy generation to the stellar core, but assumed it occurred within each layer throughout the star. He later revised his theory to include point source models to calculate the effect of energy generation within the stellar core, only to find that it did not make a substantial difference to his conclusions regarding radiative transfer and opacity.<sup>29</sup>

Jeans also had objections with the steady-state models which Eddington used in his investigations. Jeans' application of the contraction hypothesis to account for the generation of energy in a star shows the star to be constantly changing and readjusting its equilibrium. The contraction produced heat to overcome gravity which is increasing as the star becomes smaller. Thus he does not perceive the star as being in a steady state and objects to Eddington's use of steady state equations. But Eddington replied to this criticism by arguing that he never described his polytropic stars to be in a steady state because

a star can only be in a steady state if energy from some source is supplied at this rate. If it is difficult to imagine a source supplying

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<sup>28</sup> Eddington (1916): 16.

<sup>29</sup> Eddington (1925c): 409.

energy at just the right rate, that is an argument against the steady state, not against my theory.<sup>30</sup>

Eddington does not use the contraction hypothesis, believing the radiation pressure to be the dominant pressure countering the gravitational force, although he does not altogether say that there is absolutely no contraction. Because Eddington believes that energy is produced radioactively *throughout* the star, it keeps the star in radiative equilibrium, each layer exerting radiation pressure to counter gravity as well as the heat leaking slowly out from the star.

Having constructed a theory of polytropes which explained the radiative equilibrium in the interior of stars, Eddington turned to the problem of Cepheid variability this time employing his polytropic model. The periodicity of Cepheid variable stars did not vary, as they were a function of absolute magnitude and were used as time-keeping devices. In a cluster, the absolute magnitude of the Cepheid variable differs from the apparent magnitude by a constant which depends on the unknown distance of the cluster. Therefore the period-luminosity relation is given directly without the intervention of parallax, or distance measurements, and the period determines the absolute magnitude with a small error margin. The mean luminosity of the Cepheids can be used to determine a constant which was then employed in distance calculations of neighbouring stars.<sup>31</sup> Cepheid variables are pulsating stars whose periodic variations are directly related to their luminosities or absolute magnitudes (M).<sup>32</sup> Thus by measuring the *apparent* magnitude (m), the distance of the star can be measured by the following formula

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<sup>30</sup> Eddington (1917*e*): 114.

<sup>31</sup> Eddington (1926/1988): 181.

<sup>32</sup> Kragh (1996): 17. The period-luminosity relation of Cepheids variables was discovered by Henrietta Swann Leavitt at Harvard College Observatory in 1912. She found that the slower the star goes through its cycle of variation, the brighter the star.



$$m - M = 5 \log (d/10)$$

where  $m$  is the apparent magnitude,  $M$  is the absolute magnitude and  $d$  is the distance in parsecs.<sup>33</sup> In his papers, Eddington tried to find

if possible some cause maintaining the mechanical energy of pulsation against loss by dissipative forces - some method by which mechanical energy could be automatically extracted from the abundant supplies of heat at different temperatures in the star without violating the second law of thermodynamics. This might happen if the material of the star acted as the working substance of a simple thermodynamic engine..., or if the radiation pressure varied in the manner necessary to perform mechanical work.<sup>34</sup>

The answer was to employ a model of a single pulsating star. The Cepheid variables were originally assumed to be part of a binary system, but this assumption was discarded due to the distance between the stellar centres being smaller than the radius of one of the stars in the binary itself. The uniform relation between the period and the density of the star also indicated a cause intrinsic to the star. A gaseous model was employed to explain the 'varying radial velocity measuring the approach and recession of the surface presented towards the observer as the star swells and contracts.'<sup>35</sup>

A star would normally be in hydrostatic equilibrium when gravity is balanced by internal pressures and the star obeys the following equation of hydrostatic equilibrium (1) where the pressure steadily decreases as the stellar radius increases:

$$dP/dr = -GMp/r^2$$

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<sup>33</sup> Zeilik and Smith (1987): 206-208. The apparent magnitude is the brightness of a star seen from Earth. The magnitude scale for stars is a logarithmic scale such that a first-magnitude star is 100 times brighter than a sixth-magnitude star: the brighter the star, the smaller the magnitude. The luminosity of a star is related to the absolute magnitude which is the magnitude observed if the star was at a distance of 10 parsecs from the Sun. The luminosity-distance relation is given by  $m - M = 5 \log (d/10)$ .

where  $P$  is the interior pressure,  $r$  is the radius,  $M$  is the mass and  $G$  is the gravitational constant.

Stellar pulsations occur when a star is not in hydrostatic equilibrium. The star will expand due to increased gas pressure and the density will decrease until it reaches hydrostatic equilibrium and overshoots due to momentum. Gravity then takes over causing the star to contract. Momentum once again carries the contraction beyond equilibrium, and the cycle of pulsation is repeated.

The periodicity of the pulsation is related to the density due to its inward motion being a straight line and Kepler's law is used to derive the relation

$$P^2/R^3 = 4\pi^2/GM \quad (5)$$

where  $P$  is the pulsation period,  $R$  is the stellar radius and  $M$  is the stellar mass. Thus

$$P^2 \propto R^3/M \quad (6)$$

but  $M \propto \rho R^3 \quad (7)$

where  $\rho$  is the mean density and

$$P^2 \propto R^3/\rho R^3$$

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<sup>34</sup> Eddington (1926/1988): 397.

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$$\propto 1/\rho \quad (8)$$

Thus 
$$P\rho^{1/2} = \text{constant} \quad (9)$$

and the periodicity is found to be proportional to density.

### 1.1.2 The Mass-Luminosity Relation

The mass-luminosity relation was formally announced in Eddington's 1924 paper, 'On the Relation between the Mass and Luminosity of the Stars', which was published in the *Monthly Notices of the Royal Astronomical Society*, but the core idea had already appeared in his earlier papers from 1916 onwards. His papers on radiative transfer include references to his hypothesis that

the bolometric magnitude of a gaseous star is independent of its stage of evolution, and depends only on its mass<sup>36</sup>

but is fully realised in his 1924 paper. Eddington discovered theoretically that for giant gaseous stars of high mass, the luminosity was proportional to the mass, while for stars with low mass, the luminosity varied with the cube of the mass and his claims were substantiated by observational data.

We start with the equation of hydrostatic equilibrium (1)

$$dP/dr = -GM\rho/r^2$$

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<sup>35</sup> Eddington (1926/1988): 180; (1918): 177.

<sup>36</sup> Eddington (1916): 29. The bolometric magnitude is the observed luminosity of a star.

and let  $dP \rightarrow P$  and  $dr \rightarrow r$  so that

$$P = P_s - P_c = 0 - P_c$$

where  $P_s$  is the surface pressure,  $P_c$  is the central pressure and  $r$  is  $R$ . Thus

$$P_c \propto M\rho/R \quad (10)$$

For a perfect gas  $P \propto \rho T$

so  $\rho T_c \propto M\rho/R$

and  $T_c \propto M/R \quad (11)$

To calculate the luminosity we use the equation of radiative equilibrium (2)

$$L = [-64\pi\sigma r^2 T^3]/[3\kappa\rho] (dT/dr)$$

where  $\sigma$  is the Stefan-Boltzmann constant,  $\kappa$  is the opacity,  $\rho$  is the density,  $r$  is radius and  $T$  = temperature.

As with the hydrostatic equilibrium equation, we approximate to get the following

$$L \propto R^2(T_c/\kappa\rho) (T_c/R)$$

$$\propto RT_c^4/\kappa\rho \quad (12)$$

$$\text{Now } \rho \propto M/R^3 \text{ so } L \propto R^4 T_c^4 / \kappa M \quad (13)$$

And when we substitute equation (11)  $T_c \propto M/R$

$$\text{we get } L \propto R^4 (M/R)^4 / \kappa M$$

$$\propto M^3 / \kappa \quad (14)$$

and we arrive at Eddington's mass - luminosity relation:

$$L \propto M^3 \quad (15)$$

where a star's luminosity is proportional to the cube of its mass.

Jeans' main objection against the mass-luminosity relation was that Eddington had used too many assumptions which he did not believe were justified, and that it was independent of the stage of stellar evolution,

I hope I may state my grounds for disbelief in Professor Eddington's results more clearly. The results are readily combined in the one result that the total emission depends only on  $M$ , and varies as  $M$ .<sup>37</sup>

Jeans accuses Eddington of saying that by just knowing the value of the stellar mass, he is able to tell the output of radiation without knowing the source of stellar

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<sup>37</sup> Jeans (1917*d*): 444; Jeans (1917*b*): 444.

energy and assuming that the star is in a steady state. Eddington agrees that it is difficult to find a source of energy which is greater than that from Helmholtz contraction, but adds that

there is a conceivable source, which was, I believe, once suggested by Mr. Jeans himself, viz. a gradual annihilation of matter by positive and negative electrons occasionally neutralising one another.<sup>38</sup>

Jeans does not seem to have been convinced by the annihilation hypothesis and does not mention it in his objections except in terms of being a last drastic measure, although in an article in *Nature*, he admits that

some years ago I suggested that the source of stellar radiation was to be found in an actual destruction of matter in a star's interior, the mechanism problem being that positive and negative electric charges fell together and annihilated one another.<sup>39</sup>

Eddington's biographer Douglas mentions that there was a priority dispute as to who had first suggested the annihilation hypothesis. There is no doubt that Jeans was the first, yet he completely discards the hypothesis in his attacks on Eddington. It is only later that he returns to his original stand and disputes Eddington's claim as to priority.<sup>40</sup> In the series of letters which were published in *Nature*, Eddington does not claim that he suggested it first.<sup>41</sup>

Jeans consents in all his papers to agree with Eddington's conclusions on the importance of radiative transfer and radiation pressure, but he cannot accept the many *assumptions* which Eddington makes regarding the source of stellar energy. Jeans feels that Eddington's assumptions go too far, and although he himself makes similar (but less extreme) ones, Jeans uses a different methodology to arrive at the same conclusions. Thus his arguments seem to be over Eddington's derivation and initial assumptions

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<sup>38</sup> Eddington (1917*d*): 445.

<sup>39</sup> Jeans (1924): 828.

<sup>40</sup> Chandrasekhar (1987): 109.

rather than the conclusions reached. Regarding Jeans' voluminous criticisms, Eddington goes so far as to say that,

He is not likely to advance our knowledge by undoing my work. He merely verifies my algebra.<sup>42</sup>

Reviewing the papers written by both Eddington and Jeans, it is possible to see the modifications which were made along the way. In his first paper on radiative transfer, Eddington uses a value of 54 for the mean molecular weight. By the second paper, he has decreased the value to 2, having absorbed the constructive criticisms that were levelled against him by Jeans and others.<sup>43</sup> Eddington had also not reached the conclusion that the composition of stellar matter could be dominated by hydrogen even by 1925, although he indicates that it could be a possibility.

Eddington's work on the mass-luminosity relation together with Jeans' discovery that atoms in stellar interiors experienced extreme electronic dissociation led to his discovery that even at high density, perfect gas laws are not violated because extreme ionisation would strip most of the electrons from the atoms in the stellar material allowing for greater compression.<sup>44</sup> The giant-dwarf theory relied on the case that the influence of mass on luminosity was small, but Eddington's discovery showed that this was not so and led to a dramatic change in the theory of stellar evolution.

The established theory of stellar evolution during the early twentieth century was the giant-dwarf theory discovered independently by Henry Norris Russell and Ejnar Hertzsprung. Their attempts to correlate stellar observations led to the formulation of

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<sup>41</sup> Douglas (1956): 68.

<sup>42</sup> Eddington (1917*e*): 114.

<sup>43</sup> Milne (1952): 24.

<sup>44</sup> Jeans (1917*c*): 36.

the Hertzsprung-Russell diagram in 1913, which plotted stellar magnitudes against spectral type.<sup>45</sup>

Eddington explains the giant-dwarf theory in the following way:

The stars start to be visible as cool red stars of type *M* with low density and enormous bulk. They contract and in obedience to Lane's condition rise in temperature, passing up the spectral series *K*, *G*, *F* to *A* and *B* - i.e. the reverse of the previously accepted order. At some stage of the contraction the density becomes too great for the perfect gas laws to apply, the rise of temperature is checked, and ultimately the star cools down again as a solid or liquid would do; in this last stage it returns down the spectral series to type *M* and ends in extinction.<sup>46</sup>

The stars are therefore divided into two groups: the low density giant stars going up the temperature scale and obeying the perfect gas laws and the high density dwarf stars that have departed from the perfect gas state and are descending the temperature scale. Because the giant stars have larger volume and surface area their luminosities are greater than those of the dwarf stars.

Eddington's theory that perfect gas conditions still hold in high density dwarf stars meant that the evolutionary scale could no longer be divided into a perfect gas and non-perfect gas state. Another explanation had to be found.

### 1.1.3 Stellar Structure

The publication of Eddington's *Internal Constitution of the Stars* in 1926 established a new foundation for theoretical astrophysics. Although it was acclaimed by the scientific community and was an instant success, criticisms did not cease to appear from the direction of Jeans.

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<sup>45</sup> DeVorkin (2000): Chapters 6 and 8. Also see DeVorkin (1977) and Smith (1977).

<sup>46</sup> Eddington (1926/1988): 7. Lane's condition states that contraction of a gas will lead to a decrease in volume causing an increase in kinetic energy and therefore a rise in temperature.



Eddington used as his standard model a polytrope which obeyed perfect gas conditions from the centre to the surface. His earlier work before 1924 was mainly focused on low density gas giants such as the Cepheids. Yet after 1924, he realised that perfect gas conditions also prevailed for high density polytropes due to the extreme ionisation suffered by gaseous atoms.<sup>47</sup>

For over fifty years, Lane and Emden's research had established that stars were gaseous spheres. In 1917 Jeans had shown that atoms in stellar interiors were in a state of extreme electronic dissociation and Eddington then added that because the size of atoms had become so small, no matter how high the density was, stars were still compatible with the perfect gas state. Eddington since then had worked on the assumption that all stars are perfect gases.

Jeans, on the contrary, argues that the hypothesis that gas laws are obeyed do not fit observed facts and therefore should be abandoned. Jeans tries to show that gaseous stars are unstable dynamically (i.e. they should collapse) and thermodynamically (i.e. they should explode), depending on the rate of energy generation, but neither phenomenon is observed. Jeans utilises his earlier work on the behaviour of rotating masses of fluid which he had undertaken after his teacher George Howard Darwin had concluded that they were stable. Jeans had shown that they could only be stable if the mass consisted of compressible fluid rather than incompressible or rigid substances. This work helped Jeans to formulate his nebular hypothesis of the solar system using the idea of a rotating compressible fluid which would change shape as it rotated and eventually break off as instability sets in.<sup>48</sup> This was another reason why Jeans states that binary stars which are broken by rotation show that stars cannot be gaseous, as

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<sup>47</sup> Eddington (1926/1988): 165.

<sup>48</sup> Crowther (1952): 94.

gaseous stars can only contract or expand, never break up. Thus Jeans' conclusions are augmented by his study of binary stars and the effect of rotation on stellar evolution.

Extending these conclusions, Jeans put forward his hypothesis that stellar substance is more like an incompressible fluid rather than a gas. The star would have a quasi-liquid core with atoms still retaining a couple of rings of electrons and therefore exerting approximately forty times the pressure than if the atoms obeyed the gas laws. Thus the core will be an unyielding base ensuring dynamical stability, and also thermodynamical stability, if the stellar energy liberation is due to radioactivity (i.e. uninfluenced by change in temperature and density).<sup>49</sup>

In 1928 Jeans announced his theory of liquid stars in a number of articles which described them as having interiors that did not obey the ideal gas laws. His monograph, *Astronomy and Cosmogony*, which was published in the same year, contained all of his work on stellar structure which he had used to argue against Eddington's theories. The theory of liquid stars diverged considerably from Eddington's polytropic model, and provoked strong reactions through letters and articles. In his review of the book, the Norwegian astronomer Svein Rosseland remarked that Jeans held views that other astrophysicists were reluctant to accept and his book did not give a fair review of this difference in opinion. He ends his review by stating that 'in the case before us the personal views of the author pervade the book to such an extent that, besides being a work of science, it must be considered also as a work of art.'<sup>50</sup> Compared to Eddington's *Internal Constitution of the Stars*, Jeans' monograph was never fully accepted by astronomers.<sup>51</sup>

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<sup>49</sup> Jeans (1928*b*): 173, (1928*a*): 136.

<sup>50</sup> Rosseland (1928): 162.

<sup>51</sup> Milne (1952): 57.

Eddington disagrees with Jeans' theory of liquid stars on two accounts. Since 1924, there had been no opposition to his theory that stars are perfect gases with densities much greater than terrestrial densities until the appearance of Jeans' liquid stars. Although both theories agree on the extent of ionisation in the stellar interior, the two differ in the size and separation of the ions present in the stars. Jeans assumes that the ions are heavier causing the gas to 'jam' (i.e. take up a larger volume). Jeans' explanation is that the presence of a large electric field would cause the atoms to expand, compared with neutral atoms, and increase in diameter. But Eddington argues that Jeans' explanation was wrong because the electric field will make the gas super-perfect instead of what Jeans predicts.

The second reason is the relation between the liberation of energy and stability. Jeans agrees with Eddington and Russell that if the rate of liberation of subatomic energy decreases as a consequence of compression, the star becomes unstable. Eddington says that if the energy increases moderately with compression, the star is stable, but if the rate is too rapid, then the star goes into pulsation (i.e. Cepheid variables). Jeans agrees to this interpretation for small stars, but if the stellar mass is double that of the sun's, then he believes that the range of stability disappears.<sup>52</sup>

Jeans accuses Eddington that his argument is not directed towards the theory's tenability or accuracy but against its inevitability. There are only two options in the central region: the gas laws are obeyed or they are not. According to Jeans, stability rules out the first and makes the second inevitable.<sup>53</sup>

This was the state of the debate when Milne decided to survey the research on stellar structure for his Bakerian Lecture in 1929. Milne had previously been working

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<sup>52</sup> Eddington (1928): 278.

<sup>53</sup> Jeans (1928*c*): 279.

with the Cambridge physicist and expert on statistical mechanics Ralph Howard Fowler on the extension of the Saha ionisation equation and the opacity in stellar atmospheres. With this important research experience behind him, Milne sought to change the theoretical structure of the stars.

## 1.2 The Eddington-Milne Controversy: 1929-1931

The reader who would like to follow the controversy on the stellar luminosity problem has probably by this time become lost in the maze.<sup>54</sup>

Milne's background was very different from that of Eddington and Jeans. We have seen that both Eddington and Jeans trod similar paths in their career, first training for the Cambridge mathematical tripos, spending a year at the Cavendish and finally assuming lecturing posts at Cambridge. Milne, on the other hand, began the mathematical tripos but his training was cut short after only two years due to the onset of the First World War. Milne was ready to join the army to fight but was dissuaded by his friend Archibald Vivian Hill who persuaded him instead to join his team of ballistics researchers, led by Ralph Howard Fowler and himself. Milne spent the next four years as part of the team and when the war was over, he felt that he could not return to Cambridge to complete his degree. But his war work was commended highly by Fowler and Hill, and on the strength of a thesis produced on this work, he was awarded a Trinity Fellowship in 1919 at the age of twenty three. He stayed in Cambridge until 1925 when he accepted a professorship at Manchester University. In 1928 he accepted the Rouse Ball Professorship for Theoretical Physics at Oxford, where he remained for the rest of his life. Milne's incomplete war degree and his conviction that he lacked full

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<sup>54</sup> Eddington in a correspondence, *Observatory* 53: 342.

mathematical training never left him, and, as we shall see, this insecurity contributed greatly to the effect his controversy with Eddington had on him.

The immediate events leading up to the Royal Society Bakerian Lecture which Milne delivered in May 1929 heralded his entrance to the astrophysical debate between Eddington and Jeans. The previous Bakerian Lecture had been delivered by Eddington himself, who had discussed the problem of interstellar matter in the galaxy. Milne decided to follow Eddington's initiative in discussing astrophysical problems by surveying the topic of stellar structure in which Eddington and Jeans' work figured prominently. He extended his own work on the importance of boundary conditions in the stellar atmosphere in controlling properties of the stellar interior. This involved some critical analysis and constructive commentary which, in Milne's view, were all part of the normal exchange within the academic arena. Prior to the lecture, Milne had sent his manuscript to Eddington because he had detected several discrepancies between his and Eddington's work. Eddington had subsequently sent back the manuscript saying that Milne's work was all wrong. In a letter to Herbert Dingle, then Secretary of the RAS, on 1 August 1930, Milne explains,

after 3 or 4 letters on each side I (believing I must be wrong) thought I detected a mistake and withdrew the whole thing. This was 4 weeks before my Bakerian lecture - I found myself with all my preparations turned to dust and ashes. I think I could hardly give better proof of an open mind than this - a mind open to correction.<sup>55</sup>

In an unpublished manuscript, Chandrasekhar describes the meeting between Eddington and Milne as disastrous. Eddington's reaction was 'adverse and indeed intolerant', suggesting that Milne should switch topics because he, Eddington, did not agree with Milne's conclusions. Eddington had indicated that perhaps Milne's paper was not ready for public display. Milne was dismayed by Eddington's show of

contempt, and 'in deference to Eddington' Milne acquiesced by changing the subject of his Bakerian Lecture, but he never forgot the humiliation and anger he had felt.<sup>56</sup>

Milne's indignation did not subside and he published his next paper in the *Monthly Notices* in November 1929, beginning

In this paper I investigate the relations between the masses, luminosities, and effective temperatures of the stars from a standpoint which is philosophically different from that adopted by Professor Eddington in his well-known writings. The main conclusions are that it is not possible to infer from the observed masses, luminosities, and temperatures that the interiors of stars are necessarily composed of perfect gas; and that it is not possible to deduce the value of the absorption coefficient for the stellar interior. Instead, we are led to infer a single definite fact concerning the internal density-distribution.<sup>57</sup>

The main points of argument which Milne raises with regard to Eddington's work are the following: there can be no correlation between mass and luminosity unless the source and mechanism of energy generation is investigated, the importance of photospheric opacity has been neglected with regard to its effect on luminosity, and that it is not possible to analyse the relationship between mass, luminosity and temperature without referring to the interior of the star.<sup>58</sup> The main aim of Milne's work is to show the importance of observable boundary conditions.<sup>59</sup> Milne then writes,

but in summer in America I talked on my ideas with Curtis and Rosseland and Shapley and they did not think them absurd, as Eddington did. So I re-examined them, and discovered the [soul] inside the mathematics. This I read in Nov. *M.N.* My paper opened with saying my philosophical stand-point was different from Eddington's and with stating two conclusions conflicting with his. For this in *M.N.* Jan. he accused me of introducing confusion into the subject, of mysticism and sophistry, and of irresponsible conjectures -

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<sup>55</sup> Letter of 1 August 1930 (Milne to Dingle), b427/D54, Milne Archive.

<sup>56</sup> Chandrasekhar, *Edward Arthur Milne: Recollections and Reflections*, (1976-1977): 7, NBL, AIP. Chandrasekhar's manuscript is derived from his own experience with Milne and supplemented by the recollections of the astronomer William M. Smart.

<sup>57</sup> Milne (1929b): 17.

<sup>58</sup> *Observatory* 53: 303. The photospheric opacity is the absorption of radiation in the outer layer of the sun.

<sup>59</sup> Milne (1930e): 4.

in return for the very sincere complements I paid him at the end of my paper. In my two letters to "Nature" I paid him compliments each time. But he has never even faced my arguments - never met them and fronted them. I cannot get him to do so. His *M.N.* Jan. reply misrepresented me so grossly that I dropped compliments and defended myself [lustily] in May *M.N.*<sup>60</sup>

### 1.2.1 Boundary Conditions and Energy Liberation

Milne's first argument against Eddington's theory is that 'the physical content of the assertion which results from a piece of mathematico-physical reasoning must be a relation between observables only' and that 'the interior of a star can *never* be directly observed.'<sup>61</sup> Milne suggests that stellar structure should be explained in terms of mechanical equilibrium rather than radiative equilibrium because there is not enough information about the stellar interior. It should be seen as a *cooling problem* with a distribution of energy sources throughout the star which will restore it to its former equilibrium. Thus the amount radiated by the star is the algebraic sum of its energy sources which will determine the luminosity. And because in any cooling problem boundary conditions are paramount, it must be so in the case of stellar structure. In Eddington's theory, subatomic energy is used to explain luminosity calculations. Milne finds this to be inadequate as nothing is known about the source of stellar energy.

Milne criticises Eddington's attempts at discussing the source of energy generation and, like Jeans, argues that it is impossible to separate the luminosity from the relative distribution of energy sources, but without justifying his opinion, and he continues to elaborate that luminosity is only dependent on the radius, to which

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<sup>60</sup> Letter of 1 August 1930 (Milne to Dingle), b427/D54, Milne Archive. The words in the square brackets are from my reading of Milne's hand-written letters. The articles referred to are Milne (1929*b*); Eddington (1930*b*); Milne (1930*a*), (1930*b*) and (1930*d*).

<sup>61</sup> Milne (1929*b*): 17-18.

Eddington retorts, 'it is strange that Milne should leave his most essential assertion unsupported'.<sup>62</sup>

What Milne is trying to do is to formulate a relation between mass, luminosity and temperature *independent* of the stellar interior. His theory describes a homologous family of stars (to which all stars belong) in mechanical equilibrium that have the same density-distribution, but with varying radii. Depending on the luminosity, the star cools and the radius adjusts itself so that the surface opacity becomes a value such that the rate of cooling is equal to the rate of energy liberation. He writes,

in this analysis no hypothesis is made as to the nature of the interior of the star; we assume only that the outer layers are gaseous and in radiative equilibrium.<sup>63</sup>

Milne goes as far as saying that where his analysis is concerned, the interior of stars could be liquid, as Jeans had suggested.<sup>64</sup> What were of paramount importance were boundary conditions. His method of deriving a model for the stellar structure is to integrate inwards starting at the surface of the star until he reaches the boundary of the stellar core. He does not see how Eddington can deduce if the surface of a star is gaseous, that its interior must also be the same. In fact, regarding Eddington's theory of polytropes, he says

I claim no originality for making use of the principle of the avoidance of unnecessary hypotheses. That principle, I need hardly remind Professor Eddington, goes back at least as far as William of Occam. ... I nowhere criticised the plausibility of Professor Eddington's physical hypotheses about stellar interiors; I merely showed that the perfect-gas hypothesis was not a necessary one.<sup>65</sup>

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<sup>62</sup> Milne (1930*b*): 708; Eddington (1930*e*): 489.

<sup>63</sup> Milne (1929*b*): 52.

<sup>64</sup> Milne (1929*b*): 24.

<sup>65</sup> Milne (1930*d*): 679.



### 1.2.2 The Problem of Opacity

Milne had argued that Eddington's methods were inadequate to study the stellar interior. One of the main difficulties which Eddington had encountered, and which Milne focuses on, is the calculation of absorption coefficients or stellar opacity. If Eddington's calculations are accepted, then his opacity formula will give the luminosity. If not, the luminosity will give the opacity of the material in the stellar interior. But in his work, Milne announces that it is not possible to infer the opacity of the stellar interior from the values for the observed mass, luminosity and temperature, and also denies the dependence of luminosity on internal opacity. Milne found that he could deduce the mass-luminosity relation using properties of the atmosphere and therefore concluded that it was the atmosphere which determined the relation.<sup>66</sup>

In Eddington's stellar model, the parameter linking opacity and energy-liberation is

$$\kappa\eta = \text{constant} \quad (16)$$

where  $\kappa$  is the opacity and  $\eta$  is the coefficient of energy liberation, and mechanical equilibrium is taken as neutral. This allows the star to adjust its radius until the rate of cooling equals luminosity, or the rate of energy liberation throughout the interior of the star.<sup>67</sup>

In his work, Milne derives a constant

$$C = P^3/\rho^4 M^2 \quad (17)$$

where  $P$  is the pressure,  $\rho$  is the density,  $M$  is the mass, or ‘a telescoped version of the structure of the star between centre and boundary’. Milne believes this constant to be the same for all homologous stars with the same density distribution.<sup>68</sup> Eddington argues that  $C$  is a fact of the photosphere, not the interior of the star and

to call  $C$  a fact about the internal density is a sophistry, apparently occasioned by Milne's philosophical doctrine that the only thing we can really know about the inside is the outside.

Having shown that the photospheric constant is ‘irrelevant’ in determining the structure of the interior, Eddington concludes by saying ‘I cannot see that he is in a position to make any suggestion on the subject.’<sup>69</sup>

Milne insists that the photospheric effects are important in determining the conditions in the interior of the star claiming that ‘the surface conditions have a completely dominant effect’ and ‘in any cooling problem boundary conditions are paramount.’<sup>70</sup> To this Eddington replies that although photospheric parameters may differ from those in the interior, the effects will disappear as you go towards the centre of the star.<sup>71</sup> In fact, Milne says that the stellar luminosity is not determined by mass, but depends on the photospheric opacity. To this Eddington replies that the photosphere does not have such a big effect on the interior conditions. In fact, if one were to

skin off the outer layer to a depth well below the photosphere and replace it by a layer of ten times the opacity. What is the change of luminosity? My answer is, that the change of the luminosity is utterly insignificant.<sup>72</sup>

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<sup>66</sup> Whitrow, MS b423/A1, Milne Archive. Milne (1930a): 454. According to Thomas G. Cowling, one of Milne's students, this only showed that the atmosphere adjusted to the interior.

<sup>67</sup> Milne (1929b): 21.

<sup>68</sup> Milne (1929b): 22.  $C$  is defined as ‘a number depending on the complete march of the density-distribution from centre to photosphere. It depends on the general architecture of the star.’

<sup>69</sup> Eddington (1930b): 286.

<sup>70</sup> Milne (1929b): 19.

<sup>71</sup> Milne (1929b): ; Eddington (1930b): 279

<sup>72</sup> *Proceedings of the RAS meetings* (1930a): 39.

Eddington's main opposition to Milne's attacks is that Milne's field of investigation is disconnected to the problem of the stellar interior,

It appears to me that in the constructive part of Milne's paper all his references to the interior of the star should be deleted, since he has no mathematical linkage between the interior and the quantities he evaluates and he has therefore no foundation even for conjectures.<sup>73</sup>

Throughout his defence against Milne's attacks, Eddington frequently comments on the confusion which Milne seems to have woven around his theory, such as redefining parameters in such a way as to completely change their meaning.<sup>74</sup> Eddington continues by clarifying certain points of Milne's derivations by 'substitut[ing] the more recognisable quantities [of variables]' and 'by stating it in plain terms some of the mysticism surrounding it may be dispelled.'<sup>75</sup> Eddington defends his own work, saying that although his assumptions may be doubtful, they are not arbitrary or speculative, something which, he says, characterises Milne's work.

### 1.2.3 Nuclear model of a star

In November 1930 Milne published 'The Analysis of Stellar Structure' which introduced a detailed theory of stellar models with compulsory degenerate cores.<sup>76</sup> His first comment, referring to Eddington's polytropic model, is that a perfect gas star in steady state is *impossible* in nature. What was needed was a star with a small, massive core of high density and temperature or a star which maintains a high density throughout its structure, either being in a 'centrally-condensed' (high luminosity) or 'collapsed' (low luminosity) state. The former refers to giants and ordinary stars, and the latter to

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<sup>73</sup> Eddington (1930*b*): 285.

<sup>74</sup> Eddington (1930*a*): 265.

<sup>75</sup> Eddington (1930*b*): 285.

<sup>76</sup> Milne (1930*e*): 4.

white dwarfs. The mechanism of energy generation itself is taken to oppose the cooling of the star rather than subatomic energy or the liberation of energy through annihilation of matter, as no explosive mechanisms are observed.<sup>77</sup>

Milne's nuclear model of a star arises from his argument that Eddington's polytropic model is unstable. Milne's reasons for instability is different from Jeans' which is vibrational and due to rotation. In Milne's model, if the rate of energy generation falls slightly, the density-distribution changes with the mass rapidly concentrating towards the centre of the star. This occurs without the radius necessarily changing. Thus the star 'tends to precipitate itself at its centre, to crystallise out so to speak, forming a core or nucleus of very dense material', and it is to this stellar nucleus to which one had to look in order to understand energy generation.<sup>78</sup>

Thus Milne brings in degeneracy and white dwarfs into the field of stellar structure. This had not been a prominent point of argument between Eddington and Jeans because quantum mechanics had just been formulated at the time *Internal Constitution of the Stars* was published in 1926. Milne's stellar model has a double configuration divided into a dense core and an extended envelope, as opposed to Eddington's single configuration of a gaseous polytrope with uniform density.<sup>79</sup>

Eddington refuses to believe that Milne's arguments about the inadequacy of Eddington's theory is valid. Eddington had, up to this point, only worked with perfect gas models. What Milne is doing is working with stellar models which do not follow from Eddington's models, and is outside his range of investigation. In doing so Milne does not dismiss his nuclear model nor Jeans' liquid stellar model.

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<sup>77</sup> When discussing 'collapsed' stars, Milne is not referring to a gravitationally collapsed star or singularity but to white dwarfs with degenerate cores. Gravitationally collapsed stars do not come into the theory until Chandrasekhar's attempts to use relativistic degeneracy to calculate the limiting mass.

<sup>78</sup> Milne (1930c): 238; (1930f): 239.

Milne is dissatisfied with the reception of his work, which, in his view, was frequently ignored at the RAS meetings, especially by Eddington who, to Milne, had missed the point and misrepresented his work. To this Milne angrily cries,

Similarly, Sir Arthur Eddington, shackled by his mass-luminosity-opacity relation, with only two degrees of freedom, has hitherto shown himself totally unconscious of the power which results from a third degree of freedom. The third degree allows us to see right through to the centre of the star. ... I recognise that Sir Arthur Eddington has dug a most valuable trench into unknown territory. But he has encountered a rocky obstacle which he cannot get round. If he would make the mental effort to scramble up the sides of the trench he would find the surrounding country totally different from what he has imagined and the obstacle entirely an underground one.<sup>80</sup>

And regarding Eddington's quip that he does not understand how Milne could possibly be correct, Milne retorts,

I have the greatest respect for Prof. Eddington, and on this account kept back my paper six months after I had formed the conclusions in it, since they differed so from his, but in spite of it, and in spite of his remarks to-day, I think that I am right.<sup>81</sup>

Jeans on the other hand finds that Milne has only recapitulated what Jeans himself had argued over ten years ago, 'in brief, I do not think it gets anywhere; I think he has found a mare's nest.'<sup>82</sup> He does not agree with Milne's criticisms of his own theory, especially the effects of ionisation on the jamming of atoms in his liquid cores. Regarding boundary conditions, he agrees with Eddington that their influence decreases considerably as you pass into the stellar interior and says,

whatever the photospheric conditions, the photospheric layers are not thick enough to make any real difference. ... Thus Milne's involved procedure of integrating inwards getting infinite or zero density, and then letting masses of unsupported gas crash to finite densities, seems to me all unnecessary; he could have assumed a finite central density to begin with and integrated outwards, and as this is the exact procedure followed in my theory I cannot

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<sup>79</sup> Milne (1932): 610.

<sup>80</sup> Milne (1930g): 308.

<sup>81</sup> *Proceedings of the RAS meetings*, (1930a): 40.

<sup>82</sup> *Proceedings of the RAS meetings*, (1931): 36.

see how our final results can be different - unless one of us makes a mistake in analysis or arithmetic.<sup>83</sup>

Jeans concludes by saying that his differences with Eddington stem not from procedure but from their opposing views of whether stellar cores are gaseous or liquid and whether the assumption that opacity is constant within a star is valid.

The ultimate aim of both Jeans and Milne was to try and convince Eddington that it was not plausible to construct a theory connecting mass and luminosity nor to calculate the opacity of a star without knowing anything about the source of energy distribution within a star. Eddington had managed to do so, realising that because he was able to calculate the observed mass-luminosity relation without investigating the liberation of energy, this latter mechanism must be sensitive to conditions interior to the star. Thus a small adjustment within the star would produce energy enough to overcome opacity and to radiate from the star.

The controversy diminished considerably after 1932, at least at the monthly RAS meetings, although the opposing theories were never conclusively proven to be right or wrong either way. The Eddington-Milne controversy soon shifted from disagreements over the mass-luminosity relation and boundary conditions to stars with degenerate cores.

### 1.3 Interpreting the Eddington-Jeans-Milne Controversy

In a book review written in 1927 for Eddington's *Stars and Atoms*, Milne commends Eddington for using his imagination in describing difficult theories,

The prospective reader may rest assured that he is not asked to listen to vague speculations ... Professor Eddington, a great theorist, shows himself also a disciplined one.<sup>84</sup>

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<sup>83</sup> Jeans (1931): 89.

Two years later, Milne was trying to convince Eddington as to the validity of his work on stellar structure after changing his Bakerian Lecture. Not encountering any success, Milne nevertheless decided to submit a paper to the *Monthly Notices*. Chandrasekhar recalls that

according to Smart [W.M., formerly Regius Professor of Astronomy at Glasgow] the original version of the paper was written with great caution and restraint. Milne gave an account of this work at the November 1929 meeting of the Royal Astronomical Society. And in the discussion which followed, Eddington said (quoting verbatim from the account of the meeting published in the *Observatory*), “it is difficult to discuss this paper. Professor Milne did not enter into detail as to why he arrives at results so widely different from my own; and my interest in the rest of the paper is dimmed because it would be absurd to pretend that I think there is the remotest chance of his being right.” And Smart told me that these public remarks of Eddington so incensed Milne that he told him (Smart) after the meeting, “I will have the devil’s blood for that.” With that remark he withdrew the paper and rewrote it for publication with its opening flamboyant sentence, “This paper brings forward considerations which compel a drastic revision of our views.”<sup>85</sup>

The Eddington-Milne controversy lasted for approximately three years and exacted a heavy toll on Milne. What to Milne had seemed a revolutionary theory in which ‘realisation of the possibility of another way of regarding the stars was a major intellectual experience’, was carelessly brushed aside by Eddington.<sup>86</sup>

Chandrasekhar discusses Milne's ‘negative’ attitude towards the controversy and says, ‘Milne appears from this time on to be motivated explicitly or implicitly to showing that Eddington was wrong in everything.’<sup>87</sup> This view can be substantiated from Milne's letters to his family and to Chandrasekhar during this period, often through

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<sup>84</sup> Milne (1927): 293.

<sup>85</sup> Chandrasekhar, ‘Edward Arthur Milne - Recollections and Reminiscence’ (1976): 8. Chandrasekhar is referring to the articles in the *Observatory* 52: 349 and the *MNRAS* 91: 4.

<sup>86</sup> Whitrow, Box b423/A1, Milne Archive.

<sup>87</sup> Chandrasekhar, ‘Edward Arthur Milne - Recollections and Reminiscence’ (1976): 8.

little comments such as falling into an ‘Eddingtonian error of inferring physical consequences from what can be called, an *incomplete* mathematical treatment.’<sup>88</sup>

But Eddington figures prominently in an autobiographical manuscript entitled ‘My Philosophy’ written by Milne in 1950 shortly before his death. The manuscript is in note form with brief entries regarding Eddington's influence and Milne's breach with him resulting in ‘researches on stellar structure owing to sheer impossibility of getting Eddington to see a point of view different from his own’. At the end there is a summary of men who had influenced Milne: Eddington, together with Rutherford, is listed as fourth in line, coming after Milne's father, A.V. Hill (who had recruited Milne to join Hill's Brigands during the First World War) and Einstein. There is no doubt that Eddington played an incredibly large part in Milne's life, their careers entwining in the fields of stellar structure and cosmology. Yet it is strange that there are hardly any letters mentioning Eddington's death in 1944, even though there are several mentioning the deaths of Fowler and Jeans.<sup>89</sup>

Several letters from Milne to his brother Geoffrey survive which mention his controversy with Eddington. In these letters, it is difficult to see how the friendship between Eddington and Milne was maintained. Regarding Eddington's criticisms, Milne writes to his brother on 23 November 1930,

The upshot of it's that Eddington through non-rigorous mathematics and faulty ideas and far-from-critical philosophy has built up a theory which is really nonsense. And he has criticised me up hill and down dale all the time I have been struggling to work out a sound theory to replace it - called my work “mysticism” “sophistry” “unfounded speculation”, “offending in basis even for conjecture”, “misguided ...”, and finally admitted grudgingly that my work could not be “dismissed

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<sup>88</sup> Letter of 29 January 1931 (Milne to Chandrasekhar), Box 427/D4, Milne Archive.

<sup>89</sup> The quotation is from MS Box 423/A5, Milne Archive. Reference to Hill's Brigands can be found in an article by Milne's daughter, Weston-Smith (1987). Milne wrote the obituaries of Fowler and Jeans for the Royal Society and a biography of Jeans which was published posthumously. See Milne (1945*b*); (1945*c*) and (1956).



offhand” - when it’s infinitely better stuff than anything he has put out. The whole theory is an object lesson to investigators not to accept uncritically the results of people with established reputations. The main danger of science today is dogmatism.<sup>90</sup>

And a few months later on 25 January 1931,

He uses very strong language about my work - calls it sophistry and mysticism, suggestions without foundation. He is getting very pontifical these days, and is touchy about his old theory, which is rotten to the core. It’s astounding that such a complete hoodwinking of the scientific world should have gone on for so long. His theorems about stars are mostly baseless conjectures.<sup>91</sup>

The issue of uncritically accepting the opinions of people with established reputations is an important theme which will be addressed in more detail when I discuss the Chandrasekhar-Eddington controversy and its aftermath. But this issue is brought up repeatedly by Milne in his correspondence with Dingle who was Secretary of the RAS during the controversy.

On 1 August 1930 Milne writes,

I feel however hurt that I have had to fight every inch of the way against misrepresentation and very ill-thought out criticism, and that Eddington has used his great authority to retard the development of disinterested research.<sup>92</sup>

And on 7 August 1931,

I have forwarded under separate cover a paper for Supp. No. of *M.N.* This is both critical [and] constructive, but I should be very grateful if you would not send it to Eddington for his reply. I had no opportunity of replying to his two papers in March *M.N.*, thought the first misrepresents me and came to an erroneous conclusion. But more than this, when I was last ... in Germany at Potsdam I was shown in proof a most outspoken article by A.S.E. for *Zeit. für Astrophys.*, quite personal in tone and grossly misrepresenting me. Naturally I cannot reply to this (and possibly shall not in any case) before publication. But as it will be published to the world for some time before any reply is possible, I see no reason why Eddington should have preferential

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<sup>90</sup>Letter of 23 November 1930 (Milne to Geoffrey), Box b423/A9, Milne Archive.

<sup>91</sup>Letter of 25 January 1931 (Milne to Geoffrey), Box b 423/A7, Milne Archive.

<sup>92</sup>Letter of 1 August 1930 (Milne to Dingle), Box b429/D54, Milne Archive.

treatment in replying to my paper. I want it to sink into people's minds on its own merits.<sup>93</sup>

And finally, Milne had been cautioned by the RAS council to curb his attack because they believed the controversy had gone on for too long. Milne was unhappy about this decision as he felt that he did not have a proper opportunity to defend himself against Eddington.

Chandrasekhar clearly believed that Milne never recovered from his confrontations with Eddington, 'an unhappy episode which, at least in my judgement, had tragic repercussions on Milne's subsequent work.'<sup>94</sup> This is made abundantly clear from Milne's biography of Jeans which was published posthumously two years after Milne's death and eight years after the deaths of Eddington and Jeans. Although supposedly a biography of Jeans, a surprisingly large part of the book is devoted to Eddington and his unfair and ruthless treatment of Jeans and Milne during their astrophysical debates. Recalling some of Eddington's attacks on Jeans, Milne writes,

This was all very unfair to Jeans, who was an honest critic, honestly expressing his difficulties. But Eddington loved to make debating-point replies, as the present writer found when he also criticized Eddington a decade later.<sup>95</sup>

It is clear that Milne is extremely vexed that Eddington's convincing style has swayed the opinions of their peers in siding with him during the controversies even though his theories may not have been scientifically convincing. He applauds Jeans for being the sole criticizer of Eddington's theory and having the guts to stand up and do it in public.

I think that even today there is much misconception amongst astronomers about the status of Eddington's theory. The tenacity with which Eddington hung on to his first ideas and declined to modify them as research and understanding progressed, coupled with the extraordinarily attractive style of his work, *The Internal Constitution*

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<sup>93</sup> Letter of 7 August 1931 (Milne to Dingle), Box b427/D55, Milne Archive.

<sup>94</sup> Chandrasekhar (1987): 83,91.

<sup>95</sup> Milne (1952): 25-26.

*of the Stars*, has been responsible for the slowness with which astronomers have been able to winnow the chaff from the grain in his work. The present writer in 1929 became sceptical of the validity of Eddington's own account of his conclusions, and when illumination came, it came with the shock of a revelation, of a sudden conversion (or anti-conversion!). Jeans was indeed wrong about the origin of Eddington's mistranslation of his mathematical work; but he had the courage to express his *malaise* as to the legitimacy of the results. It is much to be regretted that these two Titans, Eddington and Jeans, should not have co-operated in their assaults on the grand subject of stellar structure, instead of being opposed to one another, during the most constructive periods of their careers. The blame has to be divided between them. Jeans mistakenly attacked Eddington's mathematics instead of accepting his mathematics and then providing the correct interpretation; Eddington resented what he considered to be aspersions on his competency as a mathematician, and never understood the difficulties of a philosophical kind that surrounded his own interpretations of his results. ... Astronomers on the whole have favoured Eddington's side of the controversy – mistakenly, in my opinion.<sup>96</sup>

Milne discusses his altercation with Eddington when at the RAS he openly sided with Jeans. Willem de Sitter who attended the meeting was a guest of Milne's at Oxford the next day and gently chided Milne,

that *scientific controversies were not settled by taking sides*. In his courteous way he intended thus to rebuke me for my remarks at the debate and I took his remarks in this sense. He made me feel, somehow, that I had committed some impropriety, though actually my remarks had been perfectly honest and the consequence of my own mathematical investigations. However, I decided not to lend myself to any similar misconstruction on future occasions, or to appear to bid for Jeans' support- Eddington had as usual refused to listen to the nature of the criticism I was making- and so at the resumed debate of January 1931, on stellar structure I rather unwisely went out of my way to attack Jeans' own theory...

This led to Jeans' harsh criticism of Milne work and Milne recalls that 'bystanders of the debate spoke afterward of our having 'wiped the floor' with one another.'<sup>97</sup>

Although it is clear from Milne's account that he greatly respects Eddington, he cannot keep from making digs at Eddington. Even after twenty years, Milne's anger is

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<sup>96</sup> Milne (1956): 28.

still evident. Although Milne's biography of Jeans is not exactly a worthy effort in recounting Jeans' scientific career it gives an interesting, albeit extremely biased, insight into Milne's personal recollection and psychology regarding his interactions with Eddington and Jeans. Milne's scientific output, as with Eddington and Jeans, was prodigious and first class and was regarded extremely highly by his peers. But his personal memoirs strongly imply, as Chandrasekhar has often suggested, that Milne's career had been tarnished in some way by his controversy with Eddington.

### **1.3.1 The Effect of Controversies on Friendship**

It is difficult to say precisely in what way controversies take their toll on the personal friendships of the scientists as it depends on the stakes involved and on the conceptual framework in which the controversies occurred. British astronomers formed a very small community in the early twentieth century. Although observatories were scattered throughout the country, the majority of professional astronomers with university positions were mainly concentrated between Oxford, Cambridge and London, and the monthly RAS meetings kept them in touch and abreast of any new British and continental theories. Thus they kept closely in touch, exchanging correspondence, and in the case of Cambridge, dining together in hall with the majority of astronomers involved in the stellar structure controversies belonging at one time to Trinity College.

Many who have discussed the controversies between Chandrasekhar, Eddington, Jeans and Milne such as McCrea, Clive W. Kilmister, who has written on Eddington's 'Fundamental Theory', Milne's daughter Meg Weston-Smith and Chandrasekhar's widow Lalitha Chandrasekhar have insisted, when interviewed, that their friendships

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<sup>97</sup> Milne (1952): 29-30.

were unaffected. It was only in the public arena that tempers ran high. In private, their friendships apparently remained unaffected.

In a biographical article on Eddington, McCrea states that

Eddington and Jeans gave each other no quarter; they could behave in this way - and enjoy doing so - because privately they were on excellent terms.<sup>98</sup>

Chandrasekhar himself always maintained that his controversy with Eddington did not affect their personal friendship. Yet it seems unlikely that the friendships could have been maintained in the way they had been before the controversies. Certainly in the case of Milne, we can see by his correspondence that he had taken Eddington's attacks to heart. The following excerpt of a letter from Milne to Theodore Dunham, a fellow astronomer at Boston, show his excitement at the start of the controversy in 1929,

I was up at the R.A.S. on Friday and read my heretical paper on stars. Eddington said openly that it was all nonsense, but we are all very good friends.<sup>99</sup>

But by 1930, Milne's letters to his brother and to Dingle have become quite bitter. The remarks in the articles and correspondence which were published in the periodicals were certainly scathing, and their effect can be seen in Milne's correspondence.

In an unpublished, and unfinished, manuscript biography of Milne, Norriss S. Hetherington writes,

There are some circumstances that may help us understand why Milne, who did get along very well with virtually everyone else, could not tolerate Eddington. First, Milne took his personal relationships seriously, and tried very hard to do the right thing. ... When Eddington did not respond to what Milne probably viewed self-righteously as great restraint and concessions on his part, Milne would have been hurt, bewildered, and then angry, ready to place all the blame for the disagreement on Eddington. The fact that Eddington at a previous

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<sup>98</sup> McCrea (1991): 69.

<sup>99</sup> Letter of 10 November 1929 (Milne to Dunham), Box 427/D58, Milne Archive.

meeting of the Royal Astronomical Society had praised an early paper of Milne's, saying "this is a beautiful paper", had led Milne to look to Eddington for encouragement, and might well have left Milne feeling even more grievously betrayed by Eddington's later critical stance. Milne's loyalty to colleagues has been praised; evidently this was a quality he practised and expected to find in others. He might well have come to view Eddington as a devil, who in betraying Milne had also betrayed one of the most important codes of civilization, placing himself beyond the pale. The fact that the betrayal was managed by Eddington with such wit as to entertain others and sweep them along with his views would scarcely have been much consolation to Milne.<sup>100</sup>

This reference to Eddington as the 'devil' appears frequently in the material relating to the Eddington-Milne controversy. Eddington himself jestingly remarked,

Professor Milne is between the devil and the deep sea - or rather between me and the deep sea.<sup>101</sup>

And Chandrasekhar recalls that after several of Eddington's remarks at the RAS meetings Milne had angrily vowed, 'I will have the devil's blood for that.'<sup>102</sup>

Regarding Chandrasekhar's biographical manuscript of Milne, 'E.A. Milne - Recollections and Reflections', which he had written for Milne's grand-daughter Miranda Weston-Smith in 1976, Cowling, a former research student of Milne's, remarks in his letter of 2 February 1977,

I was interested in your argument that Eddington was Milne's evil genius. I am not sure that is altogether true as a continuing influence, even though it was in 1929 or so. Milne insisted to me that he and Eddington were personally good friends, even though disagreeing on matters scientific. I myself had nothing but kindness from both men; especially from Milne, of course, but Eddington also went out of his way to be helpful on more than one occasion.<sup>103</sup>

To which Chandrasekhar replies in his letter of 18 February 1977,

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<sup>100</sup> Hetherington, 'E.A. Milne Biography' MS Box 107/folder 7, Chandrasekhar Archive: 13-15.

<sup>101</sup> Proceedings of the RAS Meeting (1931): 36.

<sup>102</sup> Chandrasekhar, 'E.A. Milne - Recollections and Reflections', Oral History Archive, Niels Bohr Library: 8.

<sup>103</sup> Letter of 2 February 1977, (Cowling to Chandrasekhar), Chandrasekhar Archive, Box 13/ Folder 11.

You seem to question my view of the adverse role Eddington played in Milne's outlook on science. However, your remark that Eddington and Milne remained personal friends is not contrary evidence. Indeed, Milne himself wrote to me (I believe in 1944) that he had a very long and a very pleasant conversation with Eddington while on a visit to Cambridge, adding parenthetically 'your old enemy and mine!' ... I can substantiate the role of 'evil genius' which Eddington played in Milne's life by enumerable quotations from his letters to me and what he has said to me.<sup>104</sup>

Several sources have indicated that Eddington and Milne maintained their friendship. Possibly over the years, their heated debates may have cooled down considerably, yet Milne's letters seem to be filled with anger, and during the years of the controversy, from 1929-31, it seems unlikely that Milne regarded Eddington as a friend. There is no doubt that Milne respected Eddington and his work prior to his controversy, especially *Internal Constitution of the Stars*, but this soon changed after Eddington began to attack Milne's work.

Milne's relationship with Jeans is much calmer compared with Eddington, although Milne exchanged several heated debates with Jeans at the RAS and published several letters in *Nature*. In a letter to his brother on 25 January 1931, Milne angrily writes,

You will have seen Jeans in *Nature* on my work. His letter is almost nonsense. The fact is that he and Eddington have done so much romancing about that it irks them to be pulled down to earth with a workmanlike investigation. I know from the mental stress it caused me how original my work is, and I would willingly sacrifice all my other scientific work for it. It satisfied my own ideas of scholarly work, which neither Jeans' work nor Eddington's does. Jeans tries to bear the fruits of my investigation to bolster his own shoddy theory which no one has any use for. I fully acknowledge in my papers all places where I built on his work. As for a principle which he certainly first isolated, he abandons it himself in his own theory. The publicity has led to an annoying paragraph in the *Observer*/‘The World, Week by Week’. I believe time will show that my paper is the starting point of a new chapter in the subject. At the R.A.S. discussion, Jeans lost his temper.

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<sup>104</sup> Letter of 18 February 1977, (Chandrasekhar to Cowling), Chandrasekhar Archive, Box 13/ Folder 11.

I understand that this discussion is reported in yesterday's *Nature* but I have not seen it.<sup>105</sup>

Yet despite this, he seemed to have regained his friendship with Jeans, only ever experiencing courtesy and kindness outside the scientific debating arena, and writing Jeans' obituaries and even a biography.<sup>106</sup>

Eddington seems not to have been affected personally by his controversies. He had had a number of controversies with several leading scientists including Oliver Lodge regarding the existence of the aether soon after Einstein's paper on general relativity was published.<sup>107</sup> But his rivalry with Jeans did not stop with stellar structure. It extended to his popular work as well. Both Eddington and Jeans had published over ten popular science monographs throughout their careers ranging from stellar structure to cosmology and philosophy and kept close count of the sales of their book with respect to each other. Indeed, they were the most popular and well read scientific authors during the 1920s and 30s, giving numerous lectures and talks on radio.

Regarding the phenomenal success of one of Jeans' books, *The Mysterious Universe*, Crowther writes,

Rutherford was heard to say that Jeans had told him that 'that fellow Eddington has written a book which has sold 50,000 copies; I will write one that will sell 100,000.' And Rutherford added: 'He did.'<sup>108</sup>

### 1.3.2 On the Credibility of Astrophysics

When defending his theories, Eddington wrote the following regarding the credibility of astrophysical research,

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<sup>105</sup> Letter of 25 January 1931 (Milne to Geoffrey), Box b423/A7, Milne Archive. An account of this exchange can be found in *Proceedings of the RAS meetings* (1930a): 40; (1931): 36, and in Jeans (1931): 89.

<sup>106</sup> Hetherington, 'E.A. Milne Biography' MS Box 107/folder 7, Chandrasekhar Archive: 12-13.

<sup>107</sup> Warwick (2003): 469-475.

<sup>108</sup> Crowther (1952): 136.



I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end.<sup>109</sup>

And

the mathematical physicist is in a position of peculiar difficulty. He may work out the behaviour of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable. It is no more speculative than the binomial theorem. But when he claims a serious interest for his toy, when he suggests that his model is like something going on in Nature, he inevitably begins to speculate.<sup>110</sup>

These are remarks which Eddington addressed to fellow theoretical astrophysicists, in particular to Jeans and Milne. If astrophysicists themselves portrayed the rival theories in their field as ‘speculative’, what was the opinion of the majority of astronomers and physicists?

Eddington laments the extent of credibility which his research was often seen as reaching, yet he himself is guilty of casting these exact judgements on the work of Jeans and Milne. In fact, when Milne read his much publicised paper attacking Eddington's theory in 1929, Eddington counter-attacked by denouncing it as ‘mystical’ and a ‘sophistry’. Milne took great offence to this and spent several months defending the physical and mathematical validity of his theory. By casting such aspersions, his credibility was eroded and the majority of the RAS members did not take great notice of his theory. We can see the effect of these allusions in Milne's subsequent attempts to be taken seriously. Writing to his brother on 25 January 1931, Milne laments,

Nobody of established reputation has yet taken the trouble to read my paper through, but the younger people see it easily.<sup>111</sup>

And on 8 September 1931 he tries to convince Dingle to print his paper and explains,

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<sup>109</sup> Eddington (1920a): 19.

<sup>110</sup> Eddington (1920a): 20.

I think I did not really make clear the nature of my paper. The leading argument in it was the one put on the blackboard at the discussion meeting of R.A.S. 1st January, reported on p.34 of Feb. No. of *Observatory* in summary. The argument was categorically ignored by Eddington, and no notice was paid to it by any other speaker. In other forms I have stated it in other papers, but never until this particular mathematical demonstration. I do not feel inclined to present it again at the R.A.S. meetings, having had it ignored so often, but it is absolutely necessary to me to have it published to justify my treating configurations of variable opacity by a method different from Eddington's instead of accepting his work.

Milne is intent on proceeding constructively, yet feels that he is constantly hindered; 'I had hoped to make all future *M.N.* papers constructive, but Eddington and others will not leave me alone to develop my ideas in peace.'<sup>112</sup>

But Milne's correspondence with Dingle aims not only to legitimise his work with respect to Eddington's theory, but also to the general astronomical community which comprises the RAS. In all the letters which Milne sent, he tries to explain to Dingle the circumstances behind the controversy with Eddington and the reasons why he has written the various papers attacking Eddington's theory and defending his own. It may have been Milne's meticulous nature to make absolutely sure that things went smoothly when preparing a paper for publication, yet his continuous justification of his astrophysical work show that the RAS was not easily receptive to such theoretical and mathematical papers.

In a letter regarding a paper Chandrasekhar wanted to send to the *Monthly Notices* for publication, Milne advises Chandrasekhar to include examples in his work as he thinks the RAS secretaries will ask: 'What has this to do with astronomy?' He believes Chandrasekhar would need numerical examples to validate his paper astronomically, explaining that

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<sup>111</sup> Letter of 25 January 1931 (Milne to Geoffrey), Box b423/A7, Milne Archive.

<sup>112</sup> Letter of 8 September, 1931 (Milne to Dingle), b429/D54, Milne Archive.

I just want you to give it an ‘astronomical flavour’ to soothe the qualms of the secretaries! ... I do think the time has gone by for half-baked approximate methods in astrophysics. It wants to be done in ‘a universal form’ as found in theoretical physics.<sup>113</sup>

And in a letter sent a few years later on 18 January 1933 Milne complains that a number of his papers had been rejected for publication in the *Monthly Notices*, stating that

theoretical investigations are being regarded as slightly lacking in respectability (especially ones on stellar interiors) because there is so little agreement on the subject. I attribute this quite unjustifiable opinion to the obstructive writings of Eddington in refusing to see anything in other lines of approach.<sup>114</sup>

Chandrasekhar himself encountered similar problems in submitting papers to the RAS as can be seen from the following excerpt from a letter from McCrea, then Secretary of the RAS on 14 September 1937,

I am sure they would not mind me explaining to you that about the time your paper came we got rather a large number dealing with one part or another of the mathematics of astrophysics. There is always a feeling in the society that we normally publish too much mathematics, so we sometimes have to decline to publish a paper in which the proportion of mathematics to astronomy seems too great.<sup>115</sup>

The main source of this attitude towards theoretical astrophysics is the proportion of mathematically inclined astronomers to observational and amateur astronomers who were Fellows of the RAS. In addition to that, there were the controversies which have riddled the subject since Eddington and Jeans began their research. There had been no compromise and the arguments had become increasingly confusing and entangled. This did not help solve the problem which physicists thought was of paramount importance: the source of stellar energy. Like many astronomers who argued that observational evidence and surface conditions should be the main starting point of any theoretical investigation, physicists were not prepared to theorise on the

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<sup>113</sup> Letter of 16 January 1931 (Milne to Chandrasekhar), b427/D4, Milne Archive.

<sup>114</sup> Letter of 18 January 1933 (Milne to Chandrasekhar), b427/D16, Milne Archive.

radiative properties of the interior of a star unless its energy mechanism was known. They did not see the point in building a model unless the fundamental driving force of a star was understood. The attitude of the physicists whom Chandrasekhar encountered was not so much disapproving but *disinterested*. For example, Bohr asked Chandrasekhar during the latter's visit to Copenhagen,

I cannot be really sympathetic to work in astrophysics because the first question I want to ask when I think of the sun is where does the energy come from. You cannot tell me where the energy comes from, so how can I believe all the other things?<sup>115</sup>

And when Chandrasekhar himself had questioned his commitment to continue his research in astrophysics, Dirac, who was then acting as his supervisor in Fowler's absence, had said that if it were him, he would be working in the fields of relativity and cosmology. In Dirac's opinion, that was where all the excitement was centred.<sup>117</sup> Where the astrophysicists had argued over the method to be employed in their investigations due to the uncertainty regarding energy generation, the physicists questioned the whole investigation itself.

Because astrophysics was still a fledgling field on the fringe of astronomy, emphasis was placed more on data rather than theory. But with general relativity and quantum mechanics heralding a change in the scientific view of nature, their important theoretical implications led to the transformation of the study of stars. It was during this intense and exciting period in physics when Bohr, Pauli and Schrödinger were doing pioneering work in Germany and Dirac in Britain, that both Eddington and Jeans began publishing their research on the physics of stars, or astrophysics.

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<sup>115</sup> Letter of 14 September 1937 (McCrea to Chandrasekhar), Box 21/ folder 15, Chandrasekhar Archive.

<sup>116</sup> Wali (1991): 102.

<sup>117</sup> Wali (1991): 84; Kragh (1994): 223-4.

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Observational astronomy mainly dealt with measuring and interpreting the position and motion of celestial objects. This began to change in the nineteenth century with increasing interest in spectroscopic analysis which slowly led to the creation of observational astrophysics which focussed mainly on finding the chemical and physical properties of these objects.<sup>118</sup> Observational astrophysicists dealt with data within an already established theoretical framework. The theory has already been previously formulated by other astrophysicists. It is mainly to provide confirmation of the theory or to add further proof. The onus is placed on the collection of data rather than making any changes to the theory itself. If evidence is not forthcoming, or the data do not fit the theory, it is left to the theoretical astrophysicists to make the relevant changes; it is not the domain of the observational astrophysicists.

In theoretical astrophysics, the theory is formulated based on previous observations and theory, but this is an addition to the older theory and is therefore considered a new formulation. Often, in the case of Chandrasekhar, Eddington and Milne, observational evidence is important, but not crucial to the formulation of the theory. It is only when validating the theory that observational data is used. But this data is taken by others, not the theoreticians themselves. Focus is on the theory, not the data. Astrophysics was more like general relativity and quantum mechanics where data was used to confirm theories rather than provide a platform for theoretical formulation.<sup>119</sup> Eddington and Jeans both spent time doing some observational astronomy as part of their earlier training due to the nature of astronomy at the time (pre-general relativity and quantum mechanics), but Chandrasekhar and Milne went straight into theoretical astrophysics without any observational experience because by the time they began their

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<sup>118</sup> Meadows (1984): 3.

<sup>119</sup> Collins and Pinch (1993): 45;

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research, the framework of the field had already been constructed by Eddington and Jeans. Both their research sprang from Eddington's *Internal Constitution of the Stars* and Jeans' *Astronomy and Cosmogony*.

After the 1930s, the focus of Milne's work turned to centrally condensed configurations or stellar models with degenerate cores. Under his supervision, several of his students began to investigate the effect of degeneracy on stellar parameters, and it was through this work that Chandrasekhar got to know and work with Milne. Both Eddington and Jeans had moved slightly away from stellar astrophysics, although Eddington still contributed several articles regarding the limiting density of polytropic stars, and was still defending his theory against Milne's attacks.

In summary, the Chandrasekhar-Eddington controversy was not the first of its kind: the new field of astrophysics was rife with controversies. As we have seen, Eddington was not afraid to be involved in controversies; in fact, he seemed to enjoy participating in them. Eddington's first major astrophysical controversy was with Jeans on topics such as radiative equilibrium, the mass-luminosity relation and stellar structure where Eddington's polytropic model was pitted against Jeans' liquid stars. The Milne-Eddington controversy centred on stellar structure with Milne at first taking sides with Jeans against Eddington. However, the controversy soon became triangular with each astrophysicist criticising each other. The effect of the controversies seem not to have affected their friendships on the surface, but archival material have shown that this explanation may be too simplistic and that Milne may have been affected more deeply than either Eddington or Jeans. Astrophysics as a more theoretical and mathematical field on the fringes of astronomy lacked the credibility which established fields such as astronomy and physics enjoyed. Although interest in astrophysics was extremely high in

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the mid-1920s and early 1930s due to the Eddington-Jeans-Milne controversies, many scientists saw this more as entertainment rather than serious scientific dialogue due to the extreme confusion caused by the debates. This was the state of astrophysics when Chandrasekhar came to Cambridge to start his PhD. The Chandrasekhar-Eddington controversy must be considered within the scientific and social context given above.

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## CHAPTER TWO: White Dwarfs and Collapsed Stellar Configurations

Eddington's earlier work on white dwarf stars and Milne's growing research on centrally condensed configurations, a rival model to Eddington's polytropes, were to finally cross in Chandrasekhar's research on relativistic degeneracy and the limiting mass. Although Milne was not specifically working on stellar evolution or white dwarfs, the conditions he had laid down for the validity of his stellar models were that *all* stars had to possess degenerate cores. This was in contrast to Eddington's view that only some stars, if at all, possessed degenerate cores and that only in the white dwarf stage. Chandrasekhar's theory, as we shall see, was to be a test between the two models.

With the construction of his stellar model, Milne rapidly produced a series of papers discussing stellar models with degenerate cores or centrally collapsed configurations. These papers were supplemented by the research of a handful of young astrophysicists including some of Milne's graduate students. In the years following the first papers published by Eddington on radiative transfer (1916), the number of theoretical papers that were published on stellar structure and evolution increased considerably reaching a peak in 1924, 1926, 1930 and 1935 (27, 14, 28 and 16 papers being published in the *MNRAS* respectively) echoing the controversies that raged between Chandrasekhar, Eddington, Jeans and Milne as well as Fowler's discovery of electron degeneracy in 1926 (see figure 2.1).<sup>1</sup>

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<sup>1</sup> By stellar structure and evolution, I also refer to papers on radiative transfer, Cepheid pulsations, the internal constitution of stars, the mass-luminosity relation and electron degeneracy.



Year	Published papers on stellar astrophysics	Year	Published papers on stellar astrophysics
1915	0	1930	28
1916	2	1931	13
1917	5	1932	11
1918	3	1933	6
1919	1	1934	9
1920	3	1935	16
1921	4	1936	6
1922	6	1937	8
1923	10	1938	9
1924	27	1939	5
1925	12	1940	4
1926	14	1941	6
1927	11	1942	1
1928	8	1943	1
1929	11	1944	6

**Figure 2.1** The number of theoretical astrophysical papers on the subject of stellar structure and evolution that were published in the *MNRAS* during the years 1915-1944.

These are extremely large figures for an astronomical journal, where the majority of readers and contributors were either observational astronomers or were not involved in any theoretical (i.e. mathematical) research.<sup>2</sup> It is also remarkable that a large number of these papers were written by young researchers, not established astrophysicists such as Eddington or Jeans, who would have had less trouble getting their papers published. We can recall Milne's earlier problems in publishing his highly theoretical papers in the *MNRAS* where he was asked to make his work more relevant to observational astronomers. But with increasing interest in the subject due to the controversies which it generated, and with the backing figures of Eddington, Jeans and Milne, astrophysical

<sup>2</sup> The reports of the Annual General Meeting of the RAS are published in the February issue of the *MNRAS* each year. There is section titled 'Notes of Some Points connected with Recent Progress of Astronomy' where brief summaries of research that had been conducted over the year are grouped together under topical headings such as 'The Sun' and 'Spectroscopy'. Between 1915 and 1944, there are only three instances when research on stellar structure and evolution have been given their own topic space: (1925): **86** and (1926): **87** when the research was grouped under the headings 'Theoretical Spectroscopy on the Sun', 'Theoretical and Special Investigations' and 'On the Theoretical Side of Variable Stars' and in (1931): **92** when the research was finally given its own heading 'Stellar Structure'.

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research was slowly being accepted in the wider astronomical community with the participation of an increasing number of researchers. From the 1930s onwards, the field became less dominated by the three established astrophysicists and papers by younger researchers such as Chandrasekhar, Thomas G. Cowling, Daulut Singh Kothari, George Cunliffe McVittie and Bertha Swirles began to appear, a large number of whom had been supervised by Fowler and Milne for their doctoral research. A field which had once been the exclusive intellectual property of Eddington, Jeans and Milne, astrophysics had now, with the conceptual injection of degeneracy, become open to a new batch of astrophysicists trained in both relativity and quantum mechanics.

An exception to this new group of astrophysicists is Edmund Clifton Stoner who discovered a limiting density for white dwarf stars and the Stoner-Anderson formula for relativistic degeneracy in 1929. He was trained in the Cambridge Natural Sciences Tripos, specialising in physics and not mathematics, and his astrophysical research was taken up and encouraged by Eddington, not Milne. We can see a gradually growing division amongst the new generation towards the two opposite poles represented by Eddington and Milne. Both Chandrasekhar and Stoner are caught between the two, yet they are both finally rejected when the exact theory of the limiting mass is completed. The social dynamics between Eddington and Milne towards their younger counterparts gives a fascinating glimpse into the hierarchical system embedded in the academic astronomical community. We can see how Eddington's and Milne's attitudes towards Chandrasekhar and Stoner alter depending on their current theoretical positions, and also with regard to suspicions as to with whom they may be siding on the question of stellar structure.

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These summaries focused on the debates ranging over the mass-luminosity relation and stellar structure between Eddington, Jeans and Milne.

On his first audience with Eddington, Chandrasekhar recalls,

Though I had attended Eddington's lectures during my first year in Cambridge, the first time I had a chance to talk to him was in June 1931 after I had been awarded the Sheepshank's exhibition. I had a note from Eddington to see him at the Observatory. (I went to see him: I recall it was the day after I had received news of my mother's death and I was not in a very receptive mood.)

At the time I went to see Eddington, I had already published two papers in the *Monthly Notices*. So Eddington from the first regarded me as an ally of Milne's.<sup>3</sup>

Chandrasekhar first became acquainted with Milne through his supervisor Fowler, and although Milne did not accept Chandrasekhar's research on the limiting mass, knowing of Chandrasekhar's interest in the degenerate state of a star, he invited Chandrasekhar to work with him on his stellar model. Milne was several years younger than Eddington, and had only recently relocated from Manchester to take up his chair in mathematics at Oxford. Unlike Eddington, however, Milne enjoyed collaborating with other scientists in his research and was probably better informed about his students. Milne also ran weekly seminars within his department and also in conjunction with the physics department. Although Eddington ran a weekly Observatory Club as Director of the Cambridge Observatory, he was not interested in collaborating with younger colleagues, although he often suggested several problems for others to do, such as in the case of Stoner even though he declined to publish a joint paper.<sup>4</sup> There are also other differences between the two: Milne was a fiery individual, like quicksilver, and extremely enthusiastic about his subject and this was reflected in his lectures, whereas Eddington was more cool and collected as witnessed by many during the debates which raged in the late 1920s at the RAS in which he gave several polished performances. But

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<sup>3</sup> Handwritten reminiscence of Chandrasekhar - no date. Box2/ folder 11, Chandrasekhar Archive.

<sup>4</sup> Milne collaborated several times with Fowler on their work on ionisation in the stellar atmosphere and with Chandrasekhar on centrally collapsed configurations. Eddington, on the other hand, collaborated once with his student Alice Vibert Douglas (1922), who later became his biographer.

he was also an extremely shy man and his university lectures and private conversations which are recorded attest to this.<sup>5</sup>

An example of this business of 'side-taking' can be given by analysing several letters between Chandrasekhar and Milne during the early 1930s, when the Eddington-Milne controversy was still in full flow, in which Milne is urging Chandrasekhar to write a note pointing out mistakes which Eddington had made in one of his papers. We recall de Sitter's gentle rebuke when Milne took sides with Jeans against Eddington a few years earlier.

In a letter of 11 June 1931 regarding Eddington's assumption that the gaseous phase for a stellar model extends right to the centre of the star, which he disagrees with, Milne writes,

About publication. The whole of this has got to be pointed out by somebody and I had drafted a paper myself. But I really have not time to follow Eddington's mistakes up in all cases. I cannot send your letter to Observatory as it stands as it assumes the validity of Eddington's equation...which is wrong.

Would you like to write up a proper paper for M.N.? I had intended one for Supp. No. and had so informed the Secretaries. If you like to take the whole thing on you can acknowledge such...as are due..., if you care to. But let me know your views. I should in any case like to know whether the above analysis is correct.<sup>6</sup>

Chandrasekhar, who was then only beginning his doctoral research, was naturally reluctant to antagonise Eddington. But Milne's insistence only increases, as we seen in the following letter of 17 June 1931,

I fully appreciate your delicacy in not wishing to write the paper. But what is to be done? ... So I should be very glad if you would write it up. I have been in correspondence with the Secretary of the RAS about this mistake of ASE's and whether you write the note or I they propose to send it to him, after receiving it, so that he can reply, if he thinks it worth while.

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<sup>5</sup> Interviews with McCrea, Kilmister.

<sup>6</sup> Letter of 11 June 1931 (Milne to Chandrasekhar), D6/7 b427-9, Milne Archive.

From the point of view of general science it is important that where a mistake is made it should be pointed out, as courteously as possible, and as there has been more than enough controversy between Eddington and myself, it is desirable that it should appear that the pointing out of the error is not merely the consequence of pre-existing antipathies between ASE and myself.

I have had my time fully mapped out this summer, for other matters. So I should be very glad if you would reconsider the matter. If you had been a pupil of mine at Oxford I should have handed the matter over to you to work out as soon as I spotted the boundary pressure error and the other (more important) question-begging error.

So could you, as courteously as is consistent with truth, make a short complete paper and send it to me? You are of course quite free not to do so if you still think fit, but, I would really like you to do it.

And in the postscript, a final urge,

If you still have scruples, my name could be put at the head of the paper with yours. 'By S. Chandrasekhar and E.A. Milne.' But I do want you to write it up.<sup>7</sup>

These two letters clearly illustrate Milne's great insistence in pointing out Eddington's mistakes. As he himself admits, there had been several debates between Eddington and himself, and he did not want to show that this attack was an extension of his antagonistic attitude. As Chandrasekhar was working in the same field and was knowledgeable with regard to the Eddington-Milne controversy, one may almost say that Milne was trying to bring Chandrasekhar onto his side by pointing out Eddington's mistakes and trying to persuade Chandrasekhar to write the article indicating them. Of course, at this stage, Chandrasekhar was not personally acquainted with Eddington. Chandrasekhar only became friendly with Eddington after he had become a Fellow of Trinity in 1933. One can see that Milne was trying extremely hard to gather support against Eddington. And in this instance, he was using Chandrasekhar. Milne's experience during the debates at the RAS left him angry and embittered. He did not feel

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<sup>7</sup> Letter of 17 June 1931 (Milne to Chandrasekhar), D6/7 b427-9, Milne Archive.

vindicated, having been dismissed and accused of all sorts of mysticism and, most of all, not being taken seriously by Eddington.<sup>8</sup>

## 2.1 Early Research

The Chandrasekhar-Eddington controversy officially began in January 1935 with Eddington's public rejection of relativistic degeneracy and his accusation of absurdity regarding the limiting mass, but its roots can be traced back to the discovery of the perplexing nature of white dwarfs. Even before Chandrasekhar had cast his eyes on the problem of white dwarfs in Eddington's *Internal Constitution of the Stars*, it had piqued the interest of several observational as well as theoretical astronomers, astrophysicists and physicists.

### 2.1.1 The Discovery of White Dwarf Stars

For the road to a knowledge of the stars leads through the atom; and important knowledge of the atom has been reached through the stars.<sup>9</sup>

The most famous example of a white dwarf star is the faint companion of Sirius, the brightest star in our sky. Although its existence was predicted by Friedrich Wilhelm Bessel in 1844 due to the irregular motion of Sirius, its companion was first observed in 1862 by Alvan Clark, an American optician, who was testing his new observing glass. The companion star's mass was comparable to that of the Sun, but was almost 6.45 magnitudes fainter. Sirius B was found to have  $4/5^{\text{th}}$  the mass of the sun but radiating only  $1/360^{\text{th}}$  of the sun's light. Because of its faint luminosity, the star was assumed to be a red star with low surface temperature.

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<sup>8</sup> Chandrasekhar (1976): 8.

<sup>9</sup> Eddington (1927): 10.

In 1914, Walter Sydney Adams of Mount Wilson obtained a photographic spectrum which revealed that Sirius B was actually a hot white star, which should have a higher luminosity than that observed. The only explanation was that it must be a very small star. But as the star emits only  $1/360^{\text{th}}$  of the sun's light, its surface area must be  $1/360^{\text{th}}$  that of the sun which gives a radius of less than  $1/19^{\text{th}}$  of the solar radius. The star was more like a planet than an ordinary star with a radius three times that of the Earth which gave a density of almost 60,000 grams per cubic centimetre. This was a startling discovery because such a high density had not been encountered before.<sup>10</sup>

Eddington writes,

The message of the Companion of Sirius when it was decoded ran: 'I am composed of material 3,000 times denser than anything you have ever come across; a ton of my material would be a little nugget that you could put in a match-box.' What reply can one make to such a message? The reply which most of us made in 1914 was - 'Shut up. Don't talk nonsense.'<sup>11</sup>

This perplexing density was explained when, in 1924, Eddington showed that although this figure was exceedingly high, it was not absurd. He argued that provided a star's temperature is sufficiently high, electrons will be stripped off atoms, and matter ionised to nuclei and free electrons. The extreme ionisation will allow close packing of the nuclei and hence high densities. The electron gas will remain a perfect gas until maximum density has been achieved, after which deviations from the ideal gas law will occur rapidly.<sup>12</sup>

Eddington suggested an observational test utilising the concept of gravitational redshift from Albert Einstein's theory of general relativity to support the density

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<sup>10</sup> Eddington (1927): 48-53; Milne (1932/1936): 4. Milne gives the density of Sirius B to be 68,000 grams per cubic centimetres and the amount of light it emits as  $1/380^{\text{th}}$  that of the sun. The density of the sun is approximately 1.5 times that of water (1 gram per cubic centimetre).

<sup>11</sup> Eddington (1927): 50.

<sup>12</sup> Eddington (1926/1988): 165-167.

calculations. The high density of Sirius B will mean that its surface gravitation will be very strong compared to terrestrial values and those from its companion star Sirius A. Spectral lines from Sirius B will experience an increase in wavelength and decrease in the frequency of light resulting in a strong shift towards the red end of the spectrum by a value which Eddington calculated to be approximately 20 kilometres per second. The following year, Adams observed a displacement of 19 kilometres per second, and the matter was settled. Much to Eddington's delight, this triumph also confirmed Einstein's third prediction in his theory of general relativity.<sup>13</sup>

In 1926 Eddington published his most widely studied scientific monograph *Internal Constitution of the Stars*. In one of the final chapters of the book Eddington discusses the problem of white dwarf stars which was still proving enigmatic. As neither their constitution nor evolutionary path was known, astronomers assumed that they were planet-sized remnants of stellar evolution. At the end of his exposition on white dwarfs, Eddington draws attention to the problem of contraction and expansion which dominate the latter stages of stellar evolution. 'So far as we know,' he writes in the *Internal Constitution of the Stars*, 'the close packing of matter is only possible so long as the temperature is great enough to ionise the material.' As its stellar energy source is depleted, a star will radiate the last remnants of its thermal energy. This results in a decrease in the thermal pressure which keeps the star balanced against its internal gravitational pressure and the star will begin to contract. But once it nears the complete exhaustion of its energy supply, the star will need to cool down and regain its normal density. In order to cool down, however, it will need to expand and work against its gravitational pressure. This will require energy which is no longer available to the star.

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<sup>13</sup> Einstein's third prediction from his theory of general relativity predicted the bending of light from a strong gravitational field. In this instance, starlight was bent by the sun's gravitational field, and thus the



In Eddington's view, the star will be in a perpetual state where it needs to cool down, but does not have enough energy to do so. He continues,

It would seem that the star will be in an awkward predicament when its supply of sub-atomic energy ultimately fails. Imagine a body continually losing heat but with insufficient energy to grow cold!<sup>14</sup>

And,

Until recently I have felt that there was a serious (or, if you like, a comic) difficulty about the ultimate fate of the white dwarfs. Their high density is only possible because of the smashing of the atoms, which in turn depends on the high temperature. It does not seem permissible to suppose that the matter can remain in this compressed state if the temperature falls. We may look forward to a time when the supply of subatomic energy fails and there is nothing to maintain the high temperature; then on cooling down, the material will return to the normal density of terrestrial solids. The star must, therefore, expand, and in order to regain a density a thousandfold less the radius must expand tenfold. Energy will be required in order to force out the material against gravity. Where is this energy to come from? An ordinary star has not enough heat energy inside it to be able to expand against gravitation to this extent; and the white dwarf can scarcely be supposed to have had sufficient foresight to make special provision for this remote demand. Thus the star may be in an awkward predicament - it will be losing heat continually *but will not have enough energy to cool down*.<sup>15</sup>

This became known as Eddington's paradox.

A few months after the publication of his book, Ralph Howard Fowler, an expert on statistical mechanics, published a short paper, 'On Dense Matter', in the *Monthly Notices of the Royal Astronomical Society*.<sup>16</sup> He was one of the few scientists at Cambridge who were interested in the new quantum mechanics being developed by Niels Bohr, Werner Heisenberg and Erwin Schrödinger, and was also Dirac's doctoral supervisor. Applying the statistics of Enrico Fermi, the Italian physicist, and Dirac,

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observed position of the stars near the vicinity of the sun would be displaced from their true positions.

<sup>14</sup> Eddington (1926/1988): 172.

<sup>15</sup> Eddington (1927): 124.

Fowler found a solution to Eddington's paradox.<sup>17</sup> He demonstrated that at such high densities, the stellar gas will be in a degenerate state and thus will not require energy to cool down. This means the following: matter in white dwarfs is in a gaseous state even at high densities due to the extreme temperatures involved leading to complete ionisation. At even higher densities, the electrons are squeezed closer together. Due to what is known as the Pauli Exclusion Principle they will be allocated a quantum cell the size of their wavelength. No two electrons in the same quantum state can occupy the same cell. However, electrons with opposite spin can occupy the same cell because they exist in different quantum states. As the density increases, the electrons are squeezed into smaller and smaller cells, decreasing their wavelength and thus increasing their frequency and hence their kinetic energy, until the lowest energy level of the gas is filled.<sup>18</sup> Consequently the cells will begin to exert a pressure countering any further contraction of the gas. This electron degeneracy pressure takes over once the stellar energy source is used up and thermal pressure decreases thereby balancing the star against gravitational contraction.<sup>19</sup>

The star can now cool down without expanding because once the degenerate state has been reached, 'the temperature then ceases to have any meaning, for the star is strictly analogous to one gigantic molecule in its lowest quantum state' writes Fowler,

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<sup>16</sup> Venkataraman (1992): 75-78. Statistical mechanics is the statistical study of the distribution of energies in a gas. In classical statistical mechanics, each particle is distinguishable unlike quantum statistical mechanics where particles are indistinguishable due to the Pauli Exclusion Principle.

<sup>17</sup> Dirac (1977): 133; Kilmister (1994): 128-129; Venkataraman (1992): 78-81. Fermi and Dirac had independently discovered a new statistics for a gas with asymmetrical wave functions that obey the Pauli Exclusion Principle where there could not be more than one particle in any energy state. Electrons with opposite spin can occupy the same cell. The Bose-Einstein statistics for a gas with symmetrical wave functions allows any number of particles to occupy one state. Thus at absolute zero particles obeying the Bose-Einstein statistics will be crowded in the lowest energy state.

<sup>18</sup> The formula  $\lambda = h/v$  shows that the wavelength  $\lambda$  is inversely proportional to the frequency  $v$ . Thus as the wavelength decreases, the frequency of the wave will increase.  $h$  is Planck's constant.

<sup>19</sup> Thorne (1994):146.

and stability is achieved.<sup>20</sup> The degenerate electrons in the gas will then effectively be at zero temperature while still possessing high energy. When an atom reaches its lowest energy state it no longer radiates, although its electrons are moving at extremely high velocities. This is also the case when a star reaches its lowest energy state. As Eddington would describe this phenomenon in the appendix to his monograph *Star and Atoms*,

If you measure temperature by radiating power its temperature is absolute zero, since the radiation is nil; if you measure temperature by the average speed of molecules its temperature is the highest attainable by matter. The final fate of the white dwarf is to become at the same time the hottest and the coldest matter in the universe. Our difficulty is doubly solved. Because the star is intensely hot it has enough energy to cool down if it wants to; because it is so intensely cold it has stopped radiating and no longer wants to grow any colder. We have described what is believed to be the final state of the white dwarf and perhaps therefore of every star. ... The binding of the atom which defies the classical conception of forces has extended to cover the star. I little imagined when this survey of Stars and Atoms was begun that it would end with a glimpse of a Star-Atom.<sup>21</sup>

Eddington welcomed Fowler's contribution as it solved his paradox, stating that

the interesting point is that his solution invokes some of the most recent developments of the quantum theory - the 'new statistics' of Einstein and Bose and the wave-theory of Schrödinger.<sup>22</sup>

Although there are no direct indications at the time, it is possible Eddington may not have been completely at ease with the introduction of quantum mechanics into his theory as he later rejects Fowler's theory. It is interesting to note Eddington's explanation of the 'new statistics' as being that of Einstein and Bose rather than that of Fermi and Dirac when degeneracy specifically requires the use of the later.<sup>23</sup> Yet quantum mechanics successfully solved the paradox without greatly changing

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<sup>20</sup> Fowler (1926): 115.

<sup>21</sup> Eddington (1927): 127.

<sup>22</sup> Eddington (1927): 127.

<sup>23</sup> Chandrasekhar comments on this in Wali (1982). He says that Eddington did not really know his quantum mechanics. This suggestion is also made by Clive Kilmister.

Eddington's theory, allowing white dwarfs to achieve a peaceful end. This new view of white dwarf stars pushed quantum mechanics firmly into the theory of stellar structure and evolution. Electron degeneracy, a central theme in quantum mechanics, was rapidly becoming a central theme in describing the latter stages of stellar evolution. Despite the incomplete state of white dwarf research, the main problems concerning energy depletion, stability, and the explanation for their high densities seem to have been solved successfully between Eddington and Fowler.

### 2.1.2 Milne and his Degenerate Stellar Core Theory

Milne's theory of compulsory degenerate stellar cores, or what he called his nuclear stellar model, grew from his rejection of Eddington's polytropic model. He found Eddington's treatment of stellar structure to lack any precise change in the equations of state which governed the interior of stars. Eddington used equations which characterised stars as gaseous following the perfect gas equations from the envelope right through to the centre, whilst Milne himself used a model where the conditions in the interior changed from one where perfect gas conditions prevailed to where they diverged in the interior, becoming degenerate, and thus requiring modified equations of state.

Recalling Milne's work on stellar structure in his Milne Lecture which he delivered at Oxford in 1979, Chandrasekhar writes,

He started with the premise - at least, he took it as a foregone conclusion - that all stars must have domains of degeneracy and that they must belong to one or the other of two classes which he called centrally condensed configurations and collapsed configurations, the distinction between them consisting mainly in the extent of the domain of degeneracy.<sup>24</sup>

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<sup>24</sup> Chandrasekhar (1987): 83.

Milne discusses his theoretical argument in his first significant paper on stellar structure, 'The Analysis of Stellar Structure' which was published in the *MNRAS* in 1930 as an alternative theory to Eddington's standard model.

Milne's treatment of the point source model and the standard stellar model (where the opacity and the rate of energy generation are constant) reach the same conclusions: the radiation pressure is too high for equilibrium and as you approach the stellar centre, the perfect gas condition must break down, leading to a 'collapsed' state.

The treatment for both models is the same. For a given mass  $M$  and opacity  $\kappa$ , a value for luminosity  $L_1$  exists such that for the condition where the luminosity  $L > L_1$ , no steady state configuration exists. We also have a value of luminosity  $L_0 (< L_1)$  so that for  $L = L_0$ , the mass at any radius  $r$ ,  $M(r)$  tends to 0 as  $r$  tends to 0.

Then for the conditions  $L_1 > L > L_0$ , the mass at radius  $r$  tends to a positive value  $M(0)$  as the radius tends to 0 and  $L_0 > L > 0$ , the mass at radius  $r$  tends to a negative value  $M(0)$  as the radius tends to 0.

For  $L_1 > L > L_0$ , the gas laws break down because the mass  $M(r)$  is contained within  $r$  thus giving the mean density enclosed within  $r$  to be  $M(r)/(4/3)\pi r^3$  which increases indefinitely as  $r$  decreases if the gas laws hold. Thus the stellar core will ultimately have a very high density where the gas laws fail. Outside this core region, the perfect gas state still holds. For a perfect gas of unlimited compressibility, we have a limiting case of a point mass  $M(0)$  at the centre.

For  $L_0 > L > 0$ , the mass tends to a negative as the radius tends to zero, therefore there must be a first zero point for the mass as the radius decreases. So Milne defines a boundary  $r = r'$  where  $M(r') = 0$  but the pressure and density are non-zero. He then constructs a steady state configuration at  $r = r'$  where an artificial, transparent supporting

surface which is spherical in shape is placed. This contains no matter but encloses a central point luminosity source  $L$ . Outside this surface is mass  $M$  which exists in a perfect gas state. As  $M(r)$  vanishes at  $r = r'$ , no other perfect gas configuration with the same mass and luminosity can exist. But no such artificial supporting surface such as the one mentioned above, can possibly exist. Thus after constructing this model, Milne removes the surface, and the mass must immediately collapse. And since no other mass-luminosity configuration exists, the mass must collapse until the perfect gas law is violated. Milne insists that the mass cannot just vanish. As the mass cannot disappear, the model establishes equilibrium with the central region of the star being in a non-perfect gaseous state. During the collapse, the external radius decreases and only a fringe remains in the perfect gas condition leaving a central region of very high density.<sup>25</sup>

In summary Milne writes,

For  $L_1 > L > L_0$  we have configurations with a dense central region surrounded by an extensive gaseous envelope; for  $L_0 > L > 0$  we have almost the whole mass in a dense state surrounded by a gaseous fringe. It follows that the diffuse density-distribution  $L = L_0$  is unstable. Let  $L$  increase above  $L_0$  ever so slightly, and the new steady-state configuration possesses a central condensation; let  $L$  decrease ever so slightly below  $L_0$ , and a collapse must occur. ... In simple English, *perfect-gas* configurations have a central condensation for  $L_1 > L > L_0$ , no condensation for  $L = L_0$ , and a hole or cavity artificially maintained for  $L_0 > L > 0$ . Hence perfect-gas configurations cannot exist for  $L_0 > L > 0$ , for the hole must tend to fill itself in, and we get “collapsed” configurations.<sup>26</sup>

Milne pursued this line of reasoning and expanded it to fulfil his theory of stellar structure. According to his theory, all stars can be divided into the two categories mentioned above. Normal density or giant and ordinary dwarf stars were classified as

<sup>25</sup> Milne (1930e): 11-13. Also see Milne (1932/1936): 26-27.

<sup>26</sup> Milne (1930e): 11-13.

centrally condensed configurations and high density or white dwarf stars as collapsed configurations. And he continues,

The explanation of the existence of very dense stars is simply that, for values of  $L$  below a certain critical value depending on  $M$  and  $\kappa$ , no perfect-gas configurations, even centrally condensed, are possible: light-pressure is too small.<sup>27</sup>

Whereas Eddington's stellar model utilises polytropes obeying the perfect gas conditions throughout the interior, in Milne's model, there is a clear demarcation between a perfect gas and a degenerate gas. At the boundary there is a change in the equation of state which governs the stellar properties such as pressure, temperature and density. Milne clearly says that his calculations are mainly on the boundary between the perfect and degenerate gas phases and do not extend to the deep interior.

My 'equations of fit' for the standard model postulate nothing about the properties of the far interior of the core. They merely involve the nature of the solution which describes the state of affairs immediately interior to the surface of demarcation.<sup>28</sup>

As you go in towards the centre of a star, the density increases infinitely. The physical conditions become drastic and the polytropic equation  $P = k\rho^{5/3}$  which Eddington uses throughout his stellar model to define the relationship between pressure, density and opacity, must break down.

The white dwarf star is an ideal example to illustrate Milne's two-phase model. The collapsed configuration within whose category it falls provides a sufficiently satisfying description of its inner structure, especially the high value of its density. Occasionally within the degenerate interior of the collapsed configuration, there is another phase consisting of an incompressible gas obeying the perfect gas law. Again, at

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<sup>27</sup> Milne (1930*e*): 13.

<sup>28</sup> Milne (1931*b*): 479.

this boundary, the equation of state must be altered to accommodate this phase-change.<sup>29</sup>

Milne's description of his model is that of a

determinate configuration of equilibrium consisting of three phases - a perfect-gas phase, a degenerate-gas phase, and an incompressible phase; at the first surface of demarcation the densities are continuous; at the second surface of demarcation they are discontinuous.<sup>30</sup>

Milne describes this discontinuous demarcation in density as almost the same as that on earth such as between the air and ocean and between the solid and liquid state in the terrestrial interior.

Milne continues to say that apart from the perfect-gas and degenerate-gas phases, there is another possibility,

Under certain circumstances the velocities of agitation of the particles may become so large as to influence the mass according to the principle of relativity. When this effect is dominant, both the equations of state previously described undergo changes. For a given density and temperature the pressure of the perfect gas becomes doubled; and the pressure of a degenerate gas obeys the formula  $p = K_2 \rho^{4/3}$  instead of  $p = K \rho^{5/3}$ . Again, at sufficiently high densities compressibility may diminish so far that to all intents and purposes the material becomes incompressible, and the equation of state becomes ' $\rho = \text{constant}$ '. Again, it is possible, following Jeans' hypothesis, that at sufficiently high temperatures matter may dissolve (reversibly) into radiation, and energy of radiation coalesce into matter; such an aggregate in dissociative equilibrium will have yet another equation of state.<sup>31</sup>

Here Milne is discussing relativistic degeneracy and concludes that investigations into these circumstances have been conducted by researchers including Chandrasekhar. Relativistic degeneracy had already become a notable factor in white dwarf research. But Milne proceeds further to suggest another state of matter - an incompressible state.

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<sup>29</sup> Milne (1930e): 35-36.

<sup>30</sup> Milne (1931b): 480.

<sup>31</sup> Milne (1932/1936): 19.



By 1932, the idea of a nova phenomenon as a possible instigator of the formation of a binary system with a white dwarf, such as Sirius A and B, has become a candidate in the search for an explanation to why such dense stars exist. With gravitational collapse due to the extreme high density of the gas and low radiation pressure, gravitational energy is liberated resulting in a sudden burst of brightness within a non-rotating stellar configuration. For a rotating model, this sudden collapse in the configuration may result in rotatory instability resulting in the break of the stellar mass by 'fission' into two detached masses. These two masses, according to Milne, need not both be in a collapsed state. One may re-expand and regain its normal density, while the other would remain a collapsed configuration with high density, such as a white dwarf.<sup>32</sup>

### **2.1.3 The Milne Brigade: Degeneracy Research amongst Milne's Students**

Milne joined the University of Oxford as Rouse Ball Professor of Mathematics in 1928, following three years at Manchester. During his years at Manchester and Oxford, he supervised the research of several graduate students including Cowling and McVittie and also collaborated with others such as Chandrasekhar, Kothari and Bertha Swirles, at Cambridge, who were all working on the theory of degenerate gas and stellar structure between the years 1929 and 1932, the height of the Eddington-Milne controversy. The controversy provided ample material for the young researchers to study and greatly stimulated their work. A wealth of new research sprung up during these years, many through the influence of Milne's first significant paper on the subject, 'The Analysis of Stellar Structure', published in 1929 which outlined his rival theory to Eddington's standard model. Although not as prolific a writer as Milne, nevertheless, the students contributed greatly to the understanding of degenerate gas and its properties,

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<sup>32</sup> Milne (1932/1936): 30.

namely, the opacity. But the primary significance of the students' contributions was to lend support to Milne and his stellar theory. They were, in effect, working on Milne's theory, not Eddington's polytropic model, and calculating solutions to establish the validity of the two-phase configuration which Milne believed defined stellar structure.

Milne refers to the work of these young researchers in his Halley Lecture of 1932 which was delivered at Oxford under the title 'White Dwarfs'. By this stage, Chandrasekhar, Kothari, Ramesh Chandra Majumdar at Göttingen and Swirles had produced quantitative results for their investigations into the low opacity of degenerate matter. Due to degeneracy, all the free electrons are occupied within their respective cells. Thus they are not free to intercept passing photons as the electrons cannot move to any other cell when they absorb energy. The degenerate electrons therefore become a transparent layer which photons can freely traverse. This would cause the opacity of the degenerate gas to decrease as photons are no longer frequently absorbed. The low opacity combined with the high conductivity characteristic of the degenerate gas prevents it from maintaining any form of temperature distribution and establishes its isothermal nature.<sup>33</sup> A model of a white dwarf can thus be constructed using Milne's collapsed configuration where a degenerate, isothermal core with low opacity is surrounded by an envelope of less dense gas with a high opacity which supports a high temperature distribution. Thus the temperature within the core is maintained at a certain temperature, and the only radiation which is allowed to leave is through the envelope. Therefore the supply of energy need not be high, and white dwarfs, according to Milne probably possess the coolest interior compared to other stars of normal density.<sup>34</sup>

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<sup>33</sup> An isothermal system is one in which the temperature remains constant and therefore no temperature distribution would exist.

<sup>34</sup> Milne (1932/1936): 17-20.

McVittie summarises Milne's and Cowling's work on two-phase or composite configurations in the following way,

<i>Perfect-gas envelope</i>	<i>Degenerate-gas zone</i>
(1) Collapsed.	Collapsed or Emden.
(2) Emden.	No degenerate-gas zone.
(3) Centrally condensed.	Centrally condensed. <sup>35</sup>

where Emden refers to the solution of Emden's equation for polytropic gas spheres which determines the stellar structure in the perfect-gas region, as promoted by Eddington. Investigation into Milne's models begin at the surface and proceeds into the interior of the star. So by analysing the envelope, the type of degenerate zone can be predicted.

An investigation into the mean absorption coefficient or opacity of a degenerate gas was conducted by Swirles in 1931. Her results showed that the opacity varied with the inverse square of the temperature, and that for a degenerate gas, absorption is affected more by an electron bound to the nucleus (bound-free transition) than by a free electron in an encounter with a positive nucleus (free-free transition). She concluded that the overall theoretical value for the mean absorption coefficient decreases when degeneracy is taken into account.<sup>36</sup>

This was an important discovery, for the two most significant factors affecting research on composite configurations were opacity of degenerate material and the rate of energy generation within the stellar core.

Neglected until this point, Kothari shows in his papers that in degenerate matter, the importance of thermal conduction increases significantly eventually eclipsing radiation as the main mode of energy transfer in a star. This drastically alters any opacity calculations which ignore conduction as the dominant form of energy flow. Kothari also

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<sup>35</sup> McVittie (1931): 68.

investigates the importance of gas pressure compared to radiation pressure in both non-relativistic and relativistic cases, as well as the effect of electrical conductivity on opacity calculations for non-degenerate and degenerate gas.<sup>37</sup> The opacity calculations begun by Eddington for non-degenerate gas was further investigated for degenerate gas via free-free electron transitions by Chandrasekhar, Majumdar and Swirles. This was superseded by the realisation that the contribution to opacity by bound-free transitions became negligible when degeneracy became sufficiently high.

Since 1916, Eddington's research had shown the importance of radiative transfer in his polytropic model of a non-degenerate perfect gas. Kothari shows that with the transition from a perfect gas state to a degenerate gas state, thermal conductivity becomes almost comparable to radiation, increasing in importance as degeneracy increases. This is something which Eddington did not believe to be important and neglected to investigate.

In all the papers published by the younger researchers, it is interesting to see that they begin their papers by mentioning Milne's recent stellar theory of composite configurations, on which their work is based. Eddington's model here takes the dogmatic position of being the standard model and of secondary importance, whilst Milne's is treated as the radical, novel approach to understanding stellar structure.

As with Milne's Halley lecture, Kothari here strictly distinguishes between non-relativistic and relativistic treatments for a degenerate gas, although he does not attempt the relativistic case in his study.

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<sup>36</sup> Swirles (1931): 857.

<sup>37</sup> Kothari (1932): 61-61.

### 2.1.4 Opposition to Milne's Centrally Condensed and Collapsed Configurations

Opposition to Milne's model arose from several quarters, including Eddington, Chandrasekhar and Cowling. Eddington refuses to budge from his standard model, although he accepts Fowler's equation of state incorporating electron degeneracy for stars approaching high density such as in the case of white dwarfs. Eddington's model for ordinary non-degenerate stars would follow the perfect gas conditions from the surface to the core, unlike Milne's model which would be constructed with a two-phase configuration. This discontinuity of density in Milne's stellar model is worrying to Eddington, who strengthens his argument by using Chandrasekhar's theory regarding high mass stars which would prevent degeneracy from setting in within the stellar core, and thus allowing the star to remain in a perfect gas state. Eddington claims there is insufficient knowledge of the possible states of matter under ordinary white dwarf pressure to be able to make statements such as those made by Milne. He writes,

Hypotheses as to the existence of dense central cores in stars have generally been associated with the theoretical investigation of degenerate matter; but it should be understood that degeneracy does not help make these high densities possible. It is an *adverse* factor. The common-sense view holds good that it is easier to obtain high density with perfect gas than with relatively incompressible material.<sup>38</sup>

Cowling, who was one of Milne's former students, completed a paper in 1931 criticising Milne's theory. Although he accepted collapsed configurations, especially in the case of white dwarfs, he did not agree with centrally condensed configurations which dictated that *all* stars with normal densities must also have degenerate cores surrounded by non-degenerate gaseous envelopes,

Since a centrally condensed core possesses a central singularity, we infer ... that a composite model of the type discussed above, of which

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<sup>38</sup> Eddington (1933): 324.

the outer layers form part of a centrally condensed perfect gas model, cannot without modification represent the stars that exist in nature.<sup>39</sup>

Cowling's attack greatly offended Milne. Cowling recalls in an interview in 1978 that

There was a discussion held at the Royal Astronomical Society in January, 1931, which was supposed to be a continuation of the argument over Milne's theory, when it was presented in the previous November.

Milne in fact presented a rather neutral account, only stressing the history of the developments. Fowler and I went out to give an account of bits where our work had impinged on Milne's. And I had by this time discovered a disproof of Milne's claim that you could round things off with a degenerate core, and mentioned this, instead of supporting Milne at the meeting.

I said I didn't think that it was impossible that one might be able to find a way of rounding things off, but it was not the way that Milne had suggested.

Regarding Milne's reaction, Cowling continues,

Well, he's already felt a certain amount of irritation about my attempts still to hang onto his apron strings, after I'd left Oxford. ... his reaction generally was, "I don't know enough about your idea" and so on. But when I'd given it in a definite form, he did blow me up, rather. And I had to appeal to Chapman who kindly proceeded to act as the intermediary and make things right between us. I was very grateful to Chapman. ... He reminded me of the various ways in which I'd been indebted to Milne, and said, 'You know that you have perhaps transgressed the usual rules a bit. Just you remember the extent to which you've been indebted to him, and I'll see if I can do anything.

With the help of Sydney Chapman, Cowling's boss at Imperial College, London, and a former Cambridge astronomer whose expertise lay in the structure of giant stars, Cowling explains how he overcame his disagreement with Milne.

Anyway, at tea before the next meeting of the Royal Astronomical Society, [Milne] called me out, and we walked around Burlington House Square and hammered things out. At any rate, we ironed out our disagreements ... by each of us emphasizing the points of agreement, rather than the points of disagreement. It wasn't a case of having to accept the other person's point of view, but rather, having to accept that whether one disagreed or not, one could be, one had to be friends.

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<sup>39</sup> Cowling (1931): 478. Here a singularity does not signify what we would describe as a black hole but a completely degenerate core.

Regarding Milne's attempt to counter his arguments, Cowling says,

Milne accepted that, so far as my analysis went, I was right. He was still hoping that one might be able, with the help of the relative transparency of degenerate matter, to be able to get somewhere. It was Chandrasekhar who established that that was no way out.

But to compensate for his lack of support, Cowling recalls that he did the following,

in my endeavour to support Milne now as much as possible, I distorted a Council Note on stellar structure. The Council of the Royal Astronomical Society prepared annually notes on advances in astronomy. They had this year a note on the theory of stellar structure, and I was asked to write it. I emphasized only the work that was being done on developing Milne's theory, and played down the extreme criticism that Eddington had produced on it, to the extent that when it was read to Council, Eddington said, 'That really ought to be rewritten.' ... I had to rewrite it, putting a balance.<sup>40</sup>

Although Cowling disagreed with some of Milne's conclusions, he obviously felt that he had *betrayed* Milne by going public with his opinions at the RAS, and in this case, so did Milne. Milne viewed Cowling as one of his supporters, whilst Cowling himself felt he should have supported Milne and was a member of Milne's brigade fighting against Eddington and his standard polytropic model.

Apart from Eddington who had, because of his refusal to accept degeneracy as a compulsory condition for the structure of all stars, opposed Milne's theory from the start it was Chandrasekhar and Stoner who finally gave the 'death blow' to Milne's two-phase configurations by his research on the relativistic effects of electron degeneracy. Milne's compulsory degenerate stellar cores would naturally entail a limiting case where degeneracy cannot set in. But within his own theory, Milne does not pursue the issue of this limit, neither does it occur within the papers of his former students. It is more of an *a priori* condition which Milne uses as the foundation for his theory. Like Eddington's

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<sup>40</sup> Cowling (1978), Oral History Archive, Niels Bohr Library. The Council Note is published in the *MNRAS*, **92**: 311.

theory which revolves around his polytropic model in a perfect gas condition, Milne's theory revolves around his two-phase configuration model where a degenerate core is surrounded by a non-degenerate envelope. These are the starting points of their theories.

Stoner and Anderson had already published their papers on the limiting density for white dwarf stars in 1929 and 1930, and Chandrasekhar did the same on the limiting mass in 1931. They all highlighted the significance of relativistic effects on electron degeneracy in their arguments. Although Milne acknowledges the existence of relativistic degeneracy which was still in its infancy, it is not an important issue in his theory. No serious pursuit along these lines occur in this period except in Stoner's two papers on the maximum density of stars which was heavily encouraged by Eddington. Chandrasekhar himself abandoned his work on the limiting mass through criticism and disinterest by Fowler and Milne, and threw himself into Milne's theory of composite configurations. At this stage, although Milne's theory has begun to look slightly shaky due to the possible existence of a critical density or mass, there is no real speculation to what such a critical value would imply to the field of stellar structure as a whole. But it was still regarded as a thorn in the side of *Milne's* theory.

## **2.2 The Stoner-Anderson Formula**

### **2.2.1 Stoner: A Small Diversion into Astrophysics**

Edmund Clifton Stoner played a small but pivotal role in early twentieth century astrophysical history. In total, he published four papers on the theory of stellar structure, mainly on the limiting density of white dwarfs and relativistic electron degeneracy, and only worked in the field between 1928 and 1933.



Born in 1899 in Surrey, Stoner entered Emmanuel College, Cambridge with a Thomasson University Scholarship, Popplewell School Leaving Scholarship and an Open Exhibition in Natural Sciences. In 1920 Stoner sat for his Part I in Botany, Chemistry and Physics in the Natural Sciences Tripos gaining a first class degree. The following year he did the same for his Part II in Physics. It is notable that there is no paper in mathematics, and although Stoner later became Professor of Theoretical Physics at Leeds, he had no formal mathematical training. Recalling his undergraduate years, Stoner once wrote that undergraduates reading physics did not attend any lectures in mathematics. He writes,

It might have been supposed that mathematics was quite incidental in physics, to be picked up and used, when necessary, as casually as a soldering iron. ... but the lack of any formal teaching by mathematicians for young physicists in their receptive undergraduate period was a considerable drawback to me later, as it must have been to many of my fellows.<sup>41</sup>

He continues by saying that the necessary mathematical study for the Part II in Physics was

by *ad hoc* study on my own ... I feel that the casual attitude about mathematics was a serious defect in the Cambridge teaching of intending physicists at the time.<sup>42</sup>

At the turn of the century, physicists at Cambridge came from a background of sound mathematical training via the Mathematical Tripos which they had to complete. Gradually the Natural Sciences Tripos became popular, and mathematics became increasingly pushed into the sidelines.<sup>43</sup>

In 1919 Stoner discovered he was diabetic, and his condition deteriorated during his postgraduate years at the Cavendish from 1921 to 1924 under the supervision of Sir

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<sup>41</sup> Bates (1969): 204. This is an excerpt taken from Stoner's 39th Guthrie Lecture of the Physical Society, *Physical Society Year Book* (1955): 24.

<sup>42</sup> Bates (1969): 207.

Ernest Rutherford. He began his doctoral research on the variance of thermionic emission due to changing gas pressures but later, under the influence of Niels Bohr's quantum theory of the atom, Stoner changed the course of his research and began a systematic quantitative investigation of X-ray absorption.<sup>44</sup>

Stoner read all he could on Bohr's theory and anything he could lay his hands on about the old quantum theory. It was in the weekend of May 9 and 10, 1924, that Stoner was struck by the realisation that electrons could not occupy orbits once they were full i.e. when the atom attains a symmetrical structure. He tentatively finished a paper on electron orbits which he sent to Rutherford who forwarded it to Ralph Howard Fowler who was then one of the few British physicists specialising in statistical mechanics and quantum theory at Cambridge. After a week of discussion, Fowler encouraged Stoner to publish his research. In October 1924, Stoner published 'The Distribution of Electrons among Atomic Levels' in the *Philosophical Magazine* which Geoffrey Cantor, in his paper on Stoner's early career as a theoretical physicist, describes as 'almost epoch-making'.<sup>45</sup> Almost epoch-making because Stoner's work, which was not altogether mathematically sophisticated, was subsequently used by Fermi and then Pauli to complete his exclusion principle once quantum mechanics and the theory of electron spin was completed.<sup>46</sup> Bohr even went so far as to say that the Pauli Exclusion Principle

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<sup>43</sup> Heilbron (1983): 266.

<sup>44</sup> Cantor (1994): 281. Bohr's atomic theory combined the different states which the electrons inhabit with spectroscopic data taken from the hydrogen atom. He realised that each line in the spectra corresponded to an energy transfer by electrons moving from one orbit to another. By measuring the differences in the spectral lines, one can calculate the different kinds of jumps which the electrons make by the amount of energy that is released.

<sup>45</sup> Cantor (1994): 286.

<sup>46</sup> A detailed account of the origins of the exclusion principle can be found in Heilbron (1983) where the significance of Stoner's contribution is assessed. See also Cantor (1994) about the early years of Stoner's career.

should have actually been called the Stoner Principle. He had done it all before Pauli except Pauli's treatment was more exact.<sup>47</sup> Cantor writes about Stoner's discovery,

Stoner's theory for the distribution of electrons was rapidly hailed as a major innovation. Sommerfeld praised it in a letter to Fowler ... Soon letter and reprints began to arrive at Leeds from leading scientists throughout Europe. Stoner was now a player in the international league of quantum theorists. However, his glory was short-lived. With characteristic brashness and incisiveness Wolfgang Pauli transformed Stoner's innovative scheme...Stoner's theory thus came to be eclipsed by the neatly formulated Exclusion Principle bearing Pauli's name.<sup>48</sup>

John Heilbron, who has written extensively about the origins of quantum mechanics, attributes Stoner's failure in reaching a complete mathematical theory for the exclusion principle as being due to his lack of mathematical training. Heilbron in fact goes as far as to say that Stoner 'knew no mathematics'. Pauli's exclusion principle was highly mathematical, as was the new quantum mechanics of Bohr, Heisenberg and Schrödinger. Stoner apparently had great difficulty understanding Pauli and his work.<sup>49</sup>

On completion of his PhD, Stoner accepted a post at Leeds where he later became Professor of Theoretical Physics in 1939, remaining there until his retirement in 1963. At Leeds, Stoner specialised in magnetic properties of various materials using quantum mechanics, thermodynamics and statistical mechanics.

Stoner's foray into theoretical astrophysics lasted only five years, within which he published four papers: two on the limiting density of white dwarfs and two on the maximum densities of stars with the help of Eddington. During that five year period, Stoner also gave several popular lectures on astronomy to students at Leeds. He was a member of the Board of Visitors of the Royal Greenwich Observatory and organised

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<sup>47</sup> Bohr (1962), Oral History Archive, Niels Bohr Library.

<sup>48</sup> Cantor (1994): 288.

<sup>49</sup> Heilbron (1983): 266.

trips to observatories and lectures on astronomy and astrophysics. But why did Stoner suddenly become interested in astrophysical research?

The two strongest factors which may have influenced Stoner's interest in the subject are Jeans' monograph, *Astronomy and Cosmogony* and Stoner's interest in quantum mechanics. *Astronomy and Cosmogony* was published in 1928 in the midst of the Eddington-Jeans controversy. This proved to have sparked Stoner's interest greatly and is quoted liberally in Stoner's first two papers on the limiting density of white dwarf stars which were published in the *Philosophical Magazine* in 1929 and 1930. Compared to Eddington's polytropic model, Stoner preferred Jeans' liquid star as being closer in structure to a real star. We can date Stoner's interest in quantum mechanics from 1922 when he had first seen Bohr lecture on his atomic theory at Cambridge. His subsequent rudimentary research on the exclusion principle and his acquaintance with Fowler may have increased his interest with respect to white dwarf stars and electron degeneracy, which Fowler had been instrumental in promoting.

Stoner was the first person to publish any research on a limiting density for white dwarf stars. Although he does not draw any conclusions from his results, nevertheless, he has moved one step beyond Eddington and Fowler's treatment of the problem of white dwarf and degenerate matter. His work predates that of Chandrasekhar, Kothari and Swirles by approximately three years, and although in his first paper he neglects the importance of relativistic effects on high velocity electrons, this is corrected in his second publication with the aid of Wilhelm Anderson of the University of Tartu. From this paper, the Stoner-Anderson formula for relativistic degeneracy was born.<sup>50</sup>

### 2.2.2 The Stoner-Anderson Formula for Relativistic Degeneracy

‘The Limiting Density in White Dwarf Stars’ was published in the *Philosophical Magazine* in 1929. Taking Sirius B as an example, Stoner proceeds to describe white dwarfs and Eddington's paradox, the problem which Eddington had encountered when trying to solve how white dwarf stars will expand to normal densities and ‘cool down’ when they do not have sufficient energy to do so. He continues by describing Fowler's application of electron degeneracy pressure which allowed matter in white dwarfs to stay at a temperature of absolute zero and still possess high energy. In his first astrophysical paper, Stoner proposes to investigate whether there may be a

limiting density due to the “jamming” of the electrons (owing to the exclusion principle which forms the basis of the Fermi statistics) independently of effects due to the appreciable size of any remaining incompletely ionized atoms.

We notice Stoner's use of the word ‘jamming’, clearly taken from Jeans' theory. Stoner describes Jeans' theory of liquid stars that have departed from the ideal gas law and consequently experience a ‘congestion of the atoms’. This congestion will afflict each electron ring in turn, but with rising temperature, ionisation will set in at each level, and when the final state (K-ring) has been ionised, complete ionisation will be achieved and the white dwarf state will be reached. But according to Jeans, there will still be a few K-ring atoms that remain to cause ‘jamming’. This jamming of the K-ring atoms, rather than the nuclei, is what causes the departure from the ideal gas law and maintains the star's stability. To this, Stoner adds,

It is this connexion that there is considerable interest in the question as to whether there is a limit to electron “congestion” (using the word in the sophisticated sense already indicated) under the gravitational conditions of the stars.<sup>51</sup>

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<sup>50</sup> Stoner (1930).

<sup>51</sup> Stoner (1929): 64-65.

Here Stoner's use of the term electron congestion clearly means electron degeneracy. Stoner is looking for a limiting density where the gravitational energy released from contraction just supplies the energy required to keep the electrons together. This occurs, he says, when the effective temperature is zero. Stoner's argument opts for gravitational energy as the significant energy source, rather than radiation, which supplies the increasing momenta of the electrons within their cells as the gas contracts. Once the gravitational energy becomes insufficient, the star can no longer decrease in size.

Stoner gives the condition for a limiting density as

$$d/dn (E_K + E_G) = 0 \quad (2.1)$$

where  $E_K$  is the total kinetic energy and  $E_G$  is the gravitational potential energy.

The number of electrons is found to be

$$\begin{aligned} n &= 2.31 \times 10^{-37} M^2 \\ &= 9.24 \times 10^{29} (M/M_\odot)^2 \end{aligned} \quad (2.2)$$

The maximum density which he finds as varying with the square of the mass is then

$$\begin{aligned} \rho &= 2.5 m_H^4 n \\ &= 3.85 \times 10^6 (M/M_\odot)^2 \end{aligned} \quad (2.3)$$

where  $m_H$  is the mean molecular weight and  $M_\odot$  is the solar mass. In this paper Stoner does not give a figure for a limiting mass.

Stoner concludes from his investigation that white dwarfs possess cores where most of their mass is concentrated and whose densities approach the limiting value. This dense core is surrounded by a more diffuse layer, becoming non-degenerate as the gas approaches the stellar surface. He continues,

On this view the central portion would be in a practically incompressible state, and so would be in the “quasi-liquid” condition which Jeans postulates as essential to the stability of stars. The essential point is that such a condition of congestion can be brought about solely on account of the “space requirements” of the electrons, and that it is not necessary to assume that there is any “jamming” of a few remaining K-ring atoms.<sup>52</sup>

Stoner begins his investigation by approaching the theory of white dwarfs using Jeans' liquid star model but by the end of the paper, his writing has taken a more critical tone. He realises that Jeans' quasi-liquid state for stability in white dwarfs can be attained solely by electron degeneracy without the need to assume any jamming of atoms as Jeans does.

Although this paper is certainly significant in understanding the structure of white dwarfs, it utilises a non-standard quasi-liquid model (remembering that Eddington's polytropic model was then considered the standard one to use and that this was a year before Milne's paper on two-phase configurations was published) and it did not focus entirely on the degenerate state of the electrons. Stoner's first paper finds a limiting density using only electron degeneracy without any relativistic considerations. The importance of relativistic effects was first provided by Wilhelm Anderson at the University of Tartu in Estonia. Born in 1890, Anderson graduated from Kazan University in 1909. Following teaching posts in Samara and Minsk, he moved to Tartu

in 1920, later becoming a *Privatdozent* of Tartu Observatory where he became the first scientist to investigate the nature of stars in relativistic astrophysics. With the rise of Hitler and the order that Germans living abroad were to return, Anderson moved to Poland remaining there until his death in 1940.<sup>53</sup>

Anderson informed Stoner of the importance of relativistic corrections which must be incorporated into his formula for the limiting density. At extremely high densities, the electrons would be pushed closer together within their cells of finite size thereby increasing their momentum and hence their kinetic energies. This means an increase in their velocities which, if it approaches the velocity of light, would invite relativistic effects to take place. Anderson found that with these corrections, the density within a degenerate stellar core approaches infinity and that the stellar mass for the limiting density becomes much smaller than Stoner's non-relativistic estimate.<sup>54</sup>

'The Equilibrium of Dense Stars' was published a year later incorporating these corrections. Stoner's new paper moves away from his previous concentration on Jeans' 'liquid star' theory because the idea of 'jamming', which is central to Jeans' theory would necessitate atomic sizes which were too big. Stoner opts for electron degeneracy pressure to sustain stability according to the Fermi-Dirac statistics. Stoner writes,

It has been pointed out by Anderson that in this calculation the effect of the relativity change of mass was neglected. He has taken this effect into account, but in a manner which seems open to criticism. His general conclusions, which seem to be correct, are that the simple expression holds provided the electron densities are not too large, but that the mass corresponding to large electron densities is smaller than that previously calculated, and that it reaches a limit. The correction becomes important for stars of mass about half that of the Sun, and so

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<sup>52</sup> Stoner (1929): 69.

<sup>53</sup> Private communication from Izold Pustynnik at Tartu Observatory (17 July 1999) who kindly translated from Russian the following article about Anderson by Sapar, A. and Feklistova, T. 'Astronomy in Tartu observatory prior to, during and after World War II' in A.I. Jeremeeva (Ed.), *Astronomy at the Sharp Turns of XXth Century History*, (1995): 254-261.

<sup>54</sup> Israel (1987): 213. See W. Anderson (1929), 'Über die Grenzdichte der Materie und der Energie', *Zeitschrift für Physik*, **56**: 851-856.



for the white dwarfs which were actually considered. The main purpose of this paper is to calculate the effect of the relativity changes of mass, using a method which seems more rigorous than that of Anderson.<sup>55</sup>

What Stoner needs to modify is his expression for the total kinetic energy at absolute zero,  $E_K$  by introducing relativistic corrections for speeds approaching the speed of light  $c$ .

Stoner first derives a condition where equilibrium cannot be sustained. He then focuses on an equilibrium configuration defined by a limiting density. By using his previous condition for a limiting density, equation (2.1),

$$d/dn (E_K + E_G) = 0$$

and substituting  $n$  for  $x$  where  $x = p_o / m_o c$  where  $p_o$  is the momentum,  $m_o$  is the mass and  $c$  is the speed of light Stoner finds,

$$\rho_o = 2.5 m_H n = 4.15 \times 10^{-24} n \quad (2.4)$$

$$\text{where } n = 8\pi/3 (m_o c/n)^2 x^3 \quad (2.5)$$

The calculations give  $n = 2.396 \times 10^{-24} M^2$  and  $\rho = 9.95 \times 10^{-6} M^2$ .

And the limiting mass is

$$M_o = 2.19 \times 10^{34}$$

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<sup>55</sup> Stoner (1930): 944.

If the solar mass  $M_{\odot} = 2 \times 10^{33}$ , then  $M_0 = 1.095 M_{\odot}$ .

Anderson's result was  $M_0 = 1.37 \times 10^{33} = 0.685 M_{\odot}$  compared to Stoner's which allows for a wider range of densities.

Although Stoner assumes uniform density in his equations, he says it is highly unlikely even in a condensed state. Stoner then goes on to define the relation between the pressure and density since pressure varies as the energy per unit volume and density as the number of electrons.

For an ideal condensed star, the distribution will be polytropic obeying the relation  $p = k \rho^{\gamma}$  as given by Eddington and Emden.

From his calculations, Stoner find that for

$$n \ll 5.9 \times 10^{29}, \rho \ll 2.4 \times 10^6 \text{ and } p = k \rho^{5/3} \quad (2.6)$$

$$n \gg 5.9 \times 10^{29}, \rho \gg 2.4 \times 10^6 \text{ and } p = k \rho^{4/3} \quad (2.7)$$

Stoner's aim in this paper was to find the limit to what he calls the gravitational kinetic equilibrium where

the limiting state occurs when the decrease in gravitational energy on contraction is equal to the increase in the total kinetic energy of the electron gas. For spheres of increasing mass the limiting density varies at first as the square of the mass, and then more rapidly, there being a limiting mass ( $2.19 \times 10^{34}$ ) above which the gravitational kinetic equilibrium considered will not occur.<sup>56</sup>

As we have seen, Stoner's entrance into astrophysics was with a paper on the limiting density of white dwarfs using Jeans' stellar theory of liquid stars rather than Eddington's polytropes, the standard stellar theory at the time. His earlier work on

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<sup>56</sup> Stoner (1930): 963.

quantum theory and his familiarity with electron degeneracy soon transcended his explanation of the limiting density due to jamming. Anderson's theoretical injection of relativistic effects into Stoner's theory culminated in the formulation of the Stoner-Anderson theory for relativistic degeneracy in 1930.

### 2.2.3 The Reception of the Stoner-Anderson Formula by Astronomers

The Stoner-Anderson formula was a turning point in the study of white dwarfs and other dense stars and provoked the interest of the astrophysicists involved in such theoretical research. It naturally drew the attention of Eddington and Milne, and correspondence with the two scientists ensued during this brief period.<sup>57</sup> Eddington was the more encouraging of the two, as in a letter to Stoner of 28 February 1932, he writes,

I have been thinking that a combination of your work and mine would make quite definite the state of the question as to upper limits to the temperature and density of a star of given mass. ... Others who have written on the subject seem to consider only the two extremes of ordinary and relativistic degeneracy, whereas we are actually most concerned with intermediate conditions. ... Whilst the existence of a critical mass may have some interest of its own it does not affect the more fundamental questions.... We have been fairly generous in upper limits, so that especially if there is an abundance of hydrogen, the critical mass is probably much greater than the sun's.

And Eddington even goes as far as to suggest that Stoner change his publishing vehicle from the *Philosophical Magazine* to the *MNRAS* because

I think it would be a good thing for some of your work to appear there (in the *MNRAS*). Astronomers don't read the *Phil. Mag.* much, and I think your results are generally more to the point than most that appear on this subject.<sup>58</sup>

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<sup>57</sup> There are three letters from Eddington to Stoner in 1932, two letters from Milne in 1931, two letters from Chandrasekhar in 1931 and drafts of three letters from Stoner to Milne in 1931. All the letters are located in the Stoner Archive at the University of Leeds.

<sup>58</sup> Letter of 28 February 1932 (Eddington to Stoner), MS333/164, Stoner Archive.

His tone is encouraging, and there is nothing hostile about his references to relativistic degeneracy or critical mass. In fact, it would seem as though Eddington wholeheartedly approved of the Stoner-Anderson formula. This may be because Eddington does not see relativistic degeneracy as an integral part to the problem he is tackling at the time. He is more concerned with Milne's compulsory degenerate stellar cores, and is looking for a limiting density which will prevent such a structure from forming, rather than worry about the consequences of relativistic degeneracy.

Stoner's articles were communicated to the RAS by Eddington and were published in the *MNRAS* in the following two years. Stoner acknowledges Eddington's help in his last paper on the subject, 'Upper Limits for Densities and Temperatures in Stars',

I am greatly indebted to Sir Arthur Eddington for suggesting the desirability of this more detailed investigation of the question of "upper limits" and for his interest in the progress of the work.<sup>59</sup>

But there may have been a more personal motive for Eddington's help. The work which was conducted in investigating the upper limits to the density and temperature was to prove that there are limits to degeneracy setting in. This idea complements Chandrasekhar's work on the limiting mass which showed, although only approximately, that for stars with a high mass, a degenerate core would be impossible. The second part to Stoner's paper is written by Eddington, who concludes that Milne's theory for a two-phase configuration cannot possibly hold.<sup>60</sup>

In his papers, Stoner is careful to stay neutral. He mentions the theories of Eddington, Jeans and Milne, and apart from calculating the limiting density, does not criticise their theories, except in the case of Jeans. Although Stoner begins his

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<sup>59</sup> Stoner (1932*b*): 670.

<sup>60</sup> Eddington (1933): 324.

astrophysical investigations on the strength of Jeans' 'liquid star' theory, he ends by stressing that electron degeneracy is probably a more accurate way of explaining stability rather than Jeans' atomic jamming.

Milne, on the other hand, is more critical of Stoner's efforts, mainly because Stoner's work reveals a limiting density beyond which Milne's theory may break down. There are two letters from Milne to Stoner that survive and drafts of three letters from Stoner to Milne which are not in the Milne Archive. These letters not only show their theoretical differences, but also reveal Stoner's 'outsider' status within the astronomical community. Regarding criticisms levelled against him about a discrepancy in the numerical results, Stoner writes to Milne on 23 January 1931,

I would not like you to regard my small excursion into astrophysics with too much scorn although the white dwarf business must be regarded rather as a 'digression'. But I need hardly say that I am following up your papers with intense interest.<sup>61</sup>

Stoner clearly feels as though he is trespassing on the intellectual property of the astrophysicists, and his informal mathematical background may have contributed to this feeling of alienation.

The main argument Milne had with Stoner's theory was that in his paper, Stoner had written the 'decrease in the volume occupied by the electron necessitates an increase in their average momentum, and if the gravitational energy is insufficient for this, then further contraction will be impossible.'<sup>62</sup> But the problem is that Milne believes that what Stoner is trying to say is that if another immediate energy source is present, contraction *may* be possible. The fundamental difference between their views is that Milne believes once the star has reached critical density, the density cannot decrease beyond it. One of the main conditions governing stellar models is that as you move

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<sup>61</sup> Letter of 23 January 1931 (Stoner to Milne), MS333/159, Stoner Archive.

towards the centre of a star, the density has to increase or stay uniform. It can never decrease. But in Stoner's theory, there is a *possibility* that the density may decrease below this critical value. Only before this happens, the star will contract to prevent it. Milne is not convinced that enough energy can be liberated from contraction to prevent this. In a letter to Chandrasekhar, Milne writes

Stoner's theory of max. density of white dwarfs cannot be right for it assumes no source of energy is available except gravitational.<sup>63</sup>

This is a problem which Eddington discusses in *Internal Constitution of the Stars*. For Eddington, the only immediate source of energy which may be available to electrons in the star is through ionisation. The whole problem centres on the possibility of the existence of  $\gamma < 4/3$  ( $\gamma$  is the ratio of the specific heat at constant pressure to the specific heat at constant volume) in order to ensure stability in a degenerate star. If a star has  $\gamma < 4/3$  and undergoes contraction to produce energy which will establish equilibrium, there must be an immediate energy source available. But Eddington does not believe a star has such a source. But he does say there is no objection to  $\gamma < 4/3$  as long as the average is above  $4/3$ . But he does not recommend this hypothesis highly.<sup>64</sup>

Milne ends his letter by saying,

Astrophysics suffers from a plethora of assumptions as it is. To import an additional principle when the principles of equilibrium alone should suffice to disclose all possible configurations of equilibrium seems to me to be complicating the subject in a retrograde way.

Followed by,

Theoretical astrophysics should seek testable theorems of this character, rather than make up theories about the stars. What the stars are really like inside we shall never know in this generation. What we can do is to leave for our successors a definite set of theorems,- the behaviour of simple systems - as a background against which future

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<sup>62</sup> Letter of 24 January 1931 (Milne to Stoner), MS333/159, Stoner Archive.

<sup>63</sup> Letter of 2 November 1930 (Milne to Chandrasekhar), Box 427/folder D3, Milne Archive.

<sup>64</sup> Eddington (1926/1988): 143.

observations can be propagated. That is the real achievement of men like Schwarzschild, ... Emden, Poincaré etc.<sup>65</sup>

These two passages seem to imply that Milne was unconvinced and slightly scornful attitude towards Stoner's theory. Not only do they attack Stoner's theory, but his attitude towards astrophysics. This attitude may be what Stoner refers to in his draft letter regarding Milne's criticism, especially as he is an outsider, and possibly because his scientific background is very different from that of the other astrophysicists. His lack of mathematical training and his occupation as a physicist were two factors which blocked his acceptance amongst astronomers.

Milne's patronising manner suggests that he would not tolerate any intrusions into his subject unless they were genuine and promote the study of astrophysics. But this may hide a personal motive for Milne as well. Stoner's theory does not support Milne's. In fact, with a limiting density, Milne's compulsory degenerate core for stars would face problems. The idea of any sort of limit would mean that there is a possibility that degenerate cores are not in fact a compulsory condition for stars.

Stoner tries to convince Milne that his paper has been misunderstood, stating that the principles behind his theory and that of Milne's, as well as the numerical results, are identical. On 28 January 1931, Milne replies again stressing that there is a problem with the theory about another possible source of energy present in the stellar configuration, apart from subatomic energy. And again he writes about the possibility of density being less than the critical density in a star, saying that

Your principle only rules out their attainment by a contraction process  
- it does not forbid their existence.<sup>66</sup>

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<sup>65</sup> Letter of 24 January 1931 (Milne to Stoner), MS333/159, Stoner Archive.

<sup>66</sup> Letter of 28 January 1931 (Milne to Stoner), MS333/159, Stoner Archive.

Milne's criticism tends to be towards the theoretical foundation on which Stoner bases his formula, namely on the energy generating mechanism of white dwarfs. Stoner places great emphasis on Helmholtz contraction rather than any subatomic energy source, an issue which had been furiously debated by Eddington, Jeans and Milne a few years earlier. Although Stoner discusses electron-proton annihilation which Jeans had put forward as a possible source of energy, he does not think it plausible in a degenerate gas.

The reception of Stoner's papers by younger researchers, notably Chandrasekhar, Kothari and Swirles is more promising. Although correspondence with Chandrasekhar only exists in the Stoner Archive, Stoner's work is mentioned several times in the various papers published on degeneracy during this period. His formula is never formally questioned, and the existence of relativistic degeneracy is accepted without any opposition.

In a letter of 15 April 1931, Chandrasekhar asks whether Stoner has seen his paper on white dwarfs in the February issue of the *Philosophical Magazine* where he 'also consider[s] the density of white dwarfs from the polytropic point of view.' Chandrasekhar goes on to say that his results are in 'complete numerical agreement with yours as I also take  $\mu=2.5m_H$ . But like you, I had also emitted  $\beta$  which appears in Milne's theory.' ( $\mu$  is the mean molecular weight of the material and  $\beta$  is the ratio of gas pressure to radiation pressure in a star.) He remarks on 'how the consideration of the relativistic effect modifies the analysis', and derives the mass-luminosity relation which gives a unique figure for the mass if  $\beta = 1$ . This mass is  $0.92 M_\odot$ . This is similar to the 'limiting mass' which Stoner had derived, but Chandrasekhar points out this does not conform with Milne's theory. Stoner's theory describes the limiting mass as the



maximum mass that a white dwarf can have for a prescribed luminosity. If the stellar mass exceeds this limiting mass, then  $\beta$  and the luminosity must be adjusted to satisfy the mass-luminosity relation. Chandrasekhar had also interpreted his results in this way, but when he consulted Milne, Chandrasekhar recalls that 'Milne regarded this as an *Eddingtonian error*.'<sup>67</sup>

Regarding the whole idea of a limiting density and, especially, relativistic degeneracy, it seems to have seeped unobstructed into the collective consciousness of the astronomical community. Eddington did not object to it, and neither did Milne. The only objection Milne had to the theory was the limiting density and mass which it produced that undermined his own theory. Without the possibility of degeneracy setting in in a massive star, his two-phase configuration could not exist.

Although discovered independently, Stoner's work did influence Chandrasekhar's work on the limiting mass. Chandrasekhar was unaware of Stoner's research when he first embarked upon his investigation, probably because the first paper was published in the *Philosophical Magazine* rather than the *MNRAS*, although he was directed to Stoner's work by his supervisor Fowler when he reached Cambridge. Regarding Anderson's work, Chandrasekhar writes to his father on 30 August 1929, 'As for my paper, as I had nearly completed writing it out, a paper by a German - Wilhelm Anderson appeared discussing the same problem. Even mathematically his treatment was identical to mine. So the satisfaction's that I was able to do it independently. I do not intend sending it for publication.'<sup>68</sup> Chandrasekhar had up until then been in India, and it was on his journey to England that he began and completed his theory. Chandrasekhar's approach differs greatly from Stoner's. While Stoner bases his work on

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<sup>67</sup> Letter of 15 April 1931 (Chandrasekhar to Stoner), MS333/159, Stoner Archive.

<sup>68</sup> Letter of 30 August 1929, (Chandrasekhar to his father), Box 3/ folder 1, Chandrasekhar Archive.

Jeans' liquid stellar model, Chandrasekhar approaches the problem from Eddington's polytropic point of view. With a background that was more mathematical than Stoner's, Chandrasekhar was better equipped to fully analyse the problem. Although Chandrasekhar abandons his work on the limiting mass to pursue another line of research on Milne's composite configuration, he returns to the subject once he has become a Fellow of Trinity where he attempts to produce an exact theory of white dwarfs with full calculations and results. By this stage, the Stoner-Anderson formula had come under attack from Eddington, and Stoner himself has departed from this field to return to physics.

### 2.3 Lev Landau and the Limiting Mass

Apart from Chandrasekhar and Stoner, there is one other significant scientist who interested himself in the study of white dwarfs and relativistic degeneracy: the Russian physicist Lev Davidovich Landau. Landau was one of the brilliant new generation of physicists that was springing up in Russia who became famous with his contributions to electromagnetic theory and general relativity. He was also a colleague of Peter Kapitsa and Victor Ambartsumian's, and had met Chandrasekhar during the latter's visit to Russia in 1934.<sup>69</sup>

Landau's research on relativistic degeneracy in white dwarfs was completed in February 1931 culminating in a paper which was published in 1932 where he derived a limiting mass of  $1.5M_{\odot}$ .<sup>70</sup> Landau's approach to this problem is again different from that of Chandrasekhar's and Stoner's. He criticises the methods used by the British astrophysicists, especially Milne, because their

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<sup>69</sup> Wali (1991): 117.

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astrophysical methods usually applied in attacking the problems of stellar structure are characterised by making physical assumptions chosen only for the sake of mathematical convenience ... and has nothing to do with reality.<sup>71</sup>

Milne's criticism of Eddington's mass-luminosity relation in the light of such methods did not seem justified to Landau. This is echoed in their attempts to explain the stellar energy source, 'some mysterious process of mutual annihilation of protons and electrons, which was never observed and has no special reason to occur in stars.' It is their theoretically approximate methods which do not satisfy Landau, and this clearly illustrates the difference in the approach taken by the astrophysicists with respect to theoretical problems compared to that of a physicist. Landau's paper scorns, what to him seems, the speculative attempts of Eddington, Milne and Jeans to explain stellar structure without actually having any observable or real evidence with which they can test their theories. So Landau attempts to confront the problem of stellar structure through stellar equilibrium considerations without the numerous assumptions that riddle the astrophysicists' work. He does not start with Eddington's polytropic theory, but looks at the connection between statistical equilibrium and free energy, where equilibrium is achieved with minimum free energy. Landau divides this free energy into two parts, the first is gravitational and is proportional to  $\rho^{1/3}$ , which he takes as equalling zero, and the second is due to the equation of state which is used, depending on whether the gas is ideal, ordinarily degenerate or relativistically degenerate. He argues that for a classical gas, the energy is proportional to  $\log \rho$  which tends to  $\infty$  as  $\rho$  tends to  $\infty$ . But this, Landau finds occurs more slowly than in the case of  $\rho^{1/3}$ , and there is always

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<sup>70</sup> Landau (1932): 287.

<sup>71</sup> Landau (1932): 285.

minimum free energy at  $\rho = \infty$ . Thus a classical ideal gas cannot achieve equilibrium as every part of its system will tend towards a point.

For cases where quantum mechanics come into play, Landau finds that the free energy is proportional to  $\rho^{2/3}$ , which is greater than the gravitational part of the free energy, and therefore the star can achieve equilibrium. For cases of great density, the velocity of electrons will rise and therefore Landau states that the relativistic theory must be applied. The energy will then be proportional to  $\rho^{1/3}$  (the same as gravitational energy) and is of the form

$$F = a\rho^{1/3} \quad (2.8)$$

For a positive  $a$ , the system will expand until the density decreases and for a negative  $a$ , the system will collapse to a point. Having established these definitions, Landau now uses Emden (and Eddington's) polytropic equation with index  $n = 3$  which gives 'an equilibrium state only for masses greater than a critical mass  $M_0 = \dots 1.5\odot$ '.<sup>72</sup> As with Chandrasekhar and Stoner, Landau realises that a different equation of state is required for more massive stars.

But like Eddington was to do later, Landau did not accept this result, explaining that as we see no such evidence of extremely massive stars greater than the critical mass behaving in any way apart from ending their lives in a stable manner, he believed that these stars must then in fact violate the laws of quantum mechanics,

For  $M > M_0$ , there exists in the whole quantum theory no cause preventing the system from collapsing to a point .... As in reality such masses exist quietly as stars and do not show any such ridiculous tendencies we must conclude that all stars heavier than  $1.5\odot$  certainly

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<sup>72</sup> Landau (1932): 286-7.

possess regions in which the laws of quantum mechanics (and therefore of quantum statistics) are violated.

But Landau continues,

As we have no reason to believe that stars can be divided into two physically different classes according to the condition  $M > \text{or} < M_0$ , we may with great probability suppose that all stars possess such pathological regions. It does not contradict the above arguments, which prove only that the condition  $M > M_0$  is sufficient (but not necessary) for the existence of such regions.<sup>73</sup>

Landau uses Bohr's arguments that the law of energy is violated in the case of relativistic quantum mechanics where ordinarily quantum mechanics breaks down to justify his conclusion for the case of relativistically degenerate stars with great densities.<sup>74</sup>

We cannot directly compare Landau's rejection of the limiting mass with that of Eddington's. Whereas one is unsure about Eddington's motives, Landau's paper specifically emphasises the necessity for *realistic* predictions and conclusions. This is one of the reasons Chandrasekhar believes Landau rejected the limiting mass.<sup>75</sup> He sweeps aside the various astrophysical theories that had been occupying the better part of the 1920s as being speculative, and he places the same restrictions on his own theory. Although he does not dismiss the idea of relativistic degeneracy, he does not accept the limiting mass, mainly because the consequences of such a limit are not visible to him. Instead, he opts for the line which Bohr takes, to reject quantum mechanics. In this sense, his reaction is similar to that of Eddington's and Milne, as both reject the quantum mechanical explanations which are used for degeneracy.

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<sup>73</sup> Landau (1932): 287. See also Wali (1991): 121 (footnote) and Israel (1987): 214 - 215. Both Wali and Israel state that Landau had independently derived Chandrasekhar's limiting mass, but had immediately rejected it.

<sup>74</sup> Landau (1932): 288.

<sup>75</sup> Greenstein, G., 'Frozen Star' (manuscript copy - no date): 367, Box 1/ folder 10, Chandrasekhar Archive.

Stoner does not cite Landau's paper in his research as it was published after Stoner had stopped contributing to the subject. There is also a possibility that Stoner's expertise as a physicist rather than an astrophysicist may have prevented him from knowing where to look for the literature compared with Chandrasekhar's position. We recall that Stoner was unaware and had to be reminded by Eddington with regard to publishing in the *MNRAS* rather than the *Philosophical Magazine*, which Eddington claimed astronomers rarely read. Chandrasekhar himself cites Landau only once in a footnote in one of the papers announcing his exact theory, although he does not mention its negative conclusion. He is credited for using the equation for relativistic degeneracy

$$P = K_2 \rho^{4/3} \quad (2.9)$$

which Chandrasekhar states was first used explicitly by him in 1931 and was later derived independently by Stoner and Landau.<sup>76</sup>

Landau is mentioned in a historical context in both Wali's and Israel's accounts of white dwarf research in the 1930s. The American physicist John Archibald Wheeler is quoted to have used the term 'Landau Limit' to describe what is now generally known as the Chandrasekhar Limit well into the 1970s. Robert M. Wald, a general relativity expert and a collaborator of Chandrasekhar's, recalls that Wheeler and his colleague at Princeton who gave a series of lectures on general relativity and black holes were

effectively giving all the credit to Landau for having derived the upper mass limit and were in effect belittling Chandrasekhar's, not directly, but indirectly. I have no reason to think it was intentional.

In the mid-1970s Wald gave a talk on black holes at Chicago in which he gave a simple derivation for the limiting mass and mentioned Landau in connection with it.

Chandrasekhar later explained to him the history behind the limiting mass ‘and pointed out his paper was well before Landau’s and was actually published before Landau’s paper was received at the journal.’ Wald felt that Chandrasekhar had been upset about this slight, but Wald does not believe that it was in any way intentional on Wheeler’s part. He has no explanation for Wheeler’s reference to Landau instead of Chandrasekhar but explains that although the term Chandrasekhar Limit was being used, it was not until interest in supernova, black holes and general relativity revived in the late 1960s and early 1970s that the term became familiar to scientists as they connected the upper mass limit for these phenomena to the limiting mass for white dwarfs.<sup>77</sup>

In summary, this chapter attempted to trace the origins of stellar research in degeneracy and the limiting mass, in particular relating to white dwarfs. We have seen that Chandrasekhar’s research was not the first in this particular area: Stoner, Anderson and Landau had reached similar conclusions independently. In addition, Milne had organised a group of researchers at Oxford who were working on the problem of degeneracy in stars. Chandrasekhar’s theory using relativistic degeneracy was therefore not such a sudden or trivial subject which Eddington could simply brush aside. With Fowler’s contribution in 1926, degeneracy research became an active area in the field of astrophysics spearheaded by Milne who encouraged Chandrasekhar’s participation in his research.

Eddington was not actively researching degeneracy at this point as he believed Fowler had settled the matter of Eddington’s paradox and the problem of white dwarfs was solved. But he was aware of the existence of relativistic degeneracy and the possibility of a limiting mass as his correspondence with Stoner indicates. It is only after

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<sup>76</sup> Chandrasekhar (1935a): 224.

<sup>77</sup> Interview with Robert M. Wald (29 June 1998), Enrico Fermi Research Institute, University of Chicago.

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Chandrasekhar's talk in January 1935 that Eddington begins his assault on relativistic degeneracy. Up until then, Eddington did not seem to see it as much of a problem even though the majority of astrophysicists were actively using the Stoner-Anderson formula. So what had changed during this period to influence Eddington's stance? This is one of the main questions I will be addressing in the following chapters.



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## CHAPTER THREE: The Controversy

In this chapter I will discuss the Chandrasekhar-Eddington controversy on relativistic degeneracy in depth. I will start by discussing the process in which Chandrasekhar came to discover the importance of relativistic degeneracy when studying white dwarfs and his search for the exact theory of the limiting mass, while at the same time he was collaborating with Milne on his theory of centrally collapsed stars. I will then focus on the crucial Royal Astronomical Society meeting in January 1935 which, with Eddington's attack on relativistic degeneracy, sparked off the controversy.

### 3.1 Chandrasekhar and the Case of the Badly Behaved Stars<sup>1</sup>

#### 3.1.1 The Discovery of the Limiting Mass

The story of Chandrasekhar's momentous discovery of the limiting mass during his long voyage from India to England has been frequently narrated in various historical introductions to stellar astrophysics textbooks, biographical articles and obituaries since his death.<sup>2</sup> The discovery was made simultaneously, but independently, with Stoner, Anderson and Landau. As I have shown in the previous chapters, scientists working within the theoretical field of astrophysics tended to work alone. Unless they were within the same geographical location, communication occurred mainly through correspondence, and often after a paper was published. Chandrasekhar, Landau and Stoner were all based in different countries (India, Soviet Union and England) and working within different areas, Landau and Stoner both being physicists and Chandrasekhar just beginning his doctoral studies in astrophysics. Can we explain the

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<sup>1</sup> Letter [undated but possibly 1931] (Cowling to Chandrasekhar), Box 13/folder 11, Chandrasekhar Archive.

sudden burst of creative research in this specialised area of stellar astrophysics at this particular time? As we shall see, the explanation can be found in the state of astrophysics in the late 1920s. We recall that Eddington had published *Internal Constitution of the Stars* in 1926 followed rapidly by Fowler's paper on electron degeneracy. These two contributions to the growing subject are soon eclipsed by the fierce controversy between Eddington and Milne which focused attention on the role of degeneracy in stellar models. Stellar astrophysics had suddenly become a popular subject of controversy and a battleground of wit and fame. The monthly debates at the RAS which were chronicled in the *MNRAS* and the *Observatory* certainly helped raise the awareness of the subject. Degeneracy was an especially new area in astrophysics which attracted younger physicists and mathematicians in Cambridge who were already professing an interest in the new and exciting field of quantum mechanics and was led by the pioneering work of Fowler to whom Chandrasekhar writes,

belongs the credit for first recognizing a field of application of the 'very new' statistics of Fermi and Dirac.<sup>3</sup>

Fowler had completed his paper on 'Dense Matter' a few months after the publication of *Internal Constitution of the Stars* in 1926. Quantum mechanics itself had only recently been formulated by Heisenberg and Schrödinger.<sup>4</sup> Fowler was one of the few physicists in Britain, and even Cambridge, who had shown an interest and studied the subject. He was the first to show that electron degeneracy was a significant pressure in maintaining stellar equilibrium for high density stars and applied this idea to the physical conditions prevalent in white dwarfs.

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<sup>2</sup> See secondary sources in bibliography. Main sources are Chandrasekhar (1993); Israel (1987); McCrea (1996); Thorne (1987); Venkataraman (1985); Wali (1982), (1991) and (1998).

<sup>3</sup> Chandrasekhar, 'Historical notes on some astrophysical problems': 14, Box1/folder 1, Chandrasekhar Archive.

Chandrasekhar's introduction to astrophysics evolved from his early interest in statistical mechanics which led to his first contact with Ralph Fowler. Chandrasekhar's family background promoted his intellectual pursuit: his parents were both educated, his father was a civil servant and his mother translated German literature into Tamil, and all of his siblings (ten including Chandrasekhar) received a university education later achieving great success in their careers in academia, the arts and medicine. In addition, his paternal grandfather was a professor of mathematics and his father's brother Sir Chadrasekhara Venkata Raman was a Fellow of the Royal Society and was awarded the Nobel Prize in physics in 1930 for the discovery of the molecular scattering of light, which was later named the Raman Effect.<sup>5</sup> His family, who were of the Brahmin caste, came from a long line of educated intellectuals and government officials.<sup>6</sup> They encouraged his interest in physics but strongly advised Chandrasekhar to succeed on his own strength rather than to rely on his uncle's influence. When Raman offered Chandrasekhar an academic position in India, just before he completed his PhD, Chandrasekhar's father sent an urgent telegram saying 'My advice keep off Raman's orbit!'<sup>7</sup> The result was that Chandrasekhar was allowed to pursue his interest in mathematics and physics, take German lessons which were vital for his quantum physics readings and was exposed to issues of the *MNRAS* that one of his relatives would bring to their house. Of his interest in quantum mechanics and electron degeneracy, Chandrasekhar writes,

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<sup>4</sup> For the history of quantum mechanics and the physicists involved, see Cassidy (1992); Forman (1971); Hendry (1984); Kragh (1990), (1999); Moore (1989); Pais (1982), (1991) and Whitaker (1996).

<sup>5</sup> Wali (1991): 60.

<sup>6</sup> Chandrasekhar interview transcript (1978): 1-3, OHA, NBL; 'Subramanya Chandrasekhar, F.R.S.' by Purasu Balakrishnan, Chandrasekhar's brother, in *Triveni*, 17 (1945): 73-85, Box 1/folder 10, Chandrasekhar Archive; and details of Chandrasekhar's family background can also be found in Wali (1991): chps. 2 and 3.

<sup>7</sup> Wali (1991): 154.

my interest in the possible role of electron degeneracy to the structure of white dwarf stars was stimulated by my meeting Arnold Sommerfeld during his visit to Madras, India, in the late fall of 1928. On that occasion Sommerfeld presented me with copies of his papers on the electron theory of metals from which even an undergraduate student (as I was then) could learn.<sup>8</sup>

This is echoed in an undated draft of a paper by Chandrasekhar with the title ‘Remarks concerning my work on the theory of white dwarfs more than fifty odd years ago. - Reply to questions posed by Dr. Harvitt.’<sup>9</sup> Sommerfeld had introduced him to his application of the Fermi-Dirac statistics in his own research and by studying his paper, Chandrasekhar was able to understand how the equation of state changes from that of a perfect gas to one where the pressure is proportional to the  $5/3^{\text{rd}}$  power of the electron density when the gas becomes degenerate. It is soon after this that he encounters Fowler’s 1926 paper on dense matter.

By the time he arrives at Cambridge, Chandrasekhar has published six papers of which three were to be published in British scientific journals in which he addresses varying problems relating to the new statistics of Fermi and Dirac.<sup>10</sup> His first foray into academic research through his study of Sommerfeld’s work, therefore, was to investigate the distribution of a degenerate electron gas. He sent his articles to Fowler, then the recognised expert on Fermi-Dirac statistics and statistical mechanics at Cambridge, who commented and saw to their publication.<sup>11</sup>

The astrophysical articles and accounts of the debates between Eddington, Jeans and Milne that were published in the *MNRAS* coupled with Chandrasekhar's meeting with Sommerfeld and his introduction to the new quantum mechanics drew Chandrasekhar's interest to the theory of electron degeneracy that was being debated

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<sup>8</sup> Chandrasekhar (1989), p. vii.

<sup>9</sup> Addenda Box 70/ folder 6, Chandrasekhar Archive.

<sup>10</sup> Letter of 4 June 1929 (Chandrasekhar to father), Box 3/ folder 1, Chandrasekhar Archive.

amongst astrophysicists. The catalyst for Chandrasekhar's discovery was Fowler's paper on the application of electron degeneracy to explain the structural stability of white dwarfs and Stoner's first paper on white dwarfs. A reference to Stoner in Chandrasekhar's second paper of 1930 indicates that he had studied Stoner's first astrophysical paper which did not include the relativistic corrections that were suggested by Anderson. This supports the conclusion that although aware of Stoner's contribution to the application of electron degeneracy in white dwarfs Chandrasekhar realised the significance of special relativistic effects on the dense gas independently of Stoner and Anderson.<sup>12</sup> In his interviews and Wali's biography, Chandrasekhar maintains that Fowler drew his attention to Stoner's two papers when Chandrasekhar visited his rooms soon after his enrolment at Cambridge. Fowler had mentioned that Stoner had been conducting research along similar lines. This may have been Stoner's relativistic paper, of which Chandrasekhar was unaware at the time, but it is highly unlikely that Chandrasekhar was unaware of Stoner's first paper.

In his papers on white dwarfs and the Fermi-Dirac statistics, Chandrasekhar does not cite any of Anderson's papers, although he is aware that Anderson had been conducting research along similar lines. In a letter to his father on 30 August 1929, Chandrasekhar writes

As for my paper, as I had nearly completed writing it out, a paper by a German - Wilhelm Anderson appeared discussing the same problem. Even mathematically his treatment was identical to mine. So the satisfaction [was] that I was able to do it independently.<sup>13</sup>

In this letter Chandrasekhar does not specify exactly to which paper he is referring but in a previous letter of 27 September to his father, he states that he was

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<sup>11</sup> Chandrasekhar (1929), (1930).

<sup>12</sup> Chandrasekhar (1930): 293, first footnote.

<sup>13</sup> Letter of 30 August 1929 (Chandrasekhar to father), Chandrasekhar Archive.

working on the problem of white dwarfs with reference to the electric charge in its interior. By this stage, Chandrasekhar has extended his work on the Fermi-Dirac statistics to the theory of white dwarfs. He adds precision to Fowler's ideas by applying the theory of polytropes and finds that white dwarfs would be polytropes of index  $3/2$ .<sup>14</sup>

We recall that Anderson informs Stoner of the importance of relativistic effects at high density for degenerate electrons after Stoner's 1929 paper, and their joint solution is published in 1930. So at this stage, Chandrasekhar is unaware of the relativistic input necessary to produce the limiting mass, and in fact, he has not ventured into this particular area of research yet.

We can follow the continuing trend in Chandrasekhar's research from the new statistics of Fermi and Dirac to its astrophysical application to white dwarfs, extending the research of Fowler, which eventually results in his discovery of the limiting mass in July 1930. Chandrasekhar corresponds with both Stoner and Anderson upon arriving at Cambridge, and his work is quoted in their papers.<sup>15</sup> Regarding their simultaneous discovery, Chandrasekhar writes

About W. Anderson's remarks on my paper. His English is rather misleading. He merely draws attention to the fact by difficult lines of reasoning - what he calls mechanical ionisation - he arrives at my final conclusion. Of course there is nothing like priority in these things. They arise in 'big' things. Neither Anderson, nor I think that the idea is fundamental. It is just a consequence we noted from different lines of reasoning that comes out from the existing conceptions about the scheme of things.<sup>16</sup>

By the time Chandrasekhar writes his paper on 'The Maximum Mass of Ideal White Dwarfs', the Stoner-Anderson formula for relativistic degeneracy has already

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<sup>14</sup> Addenda Box 70/ folder 6, Chandrasekhar Archive. Different polytropic indices indicate different types of stars.

<sup>15</sup> Letter of 10 February 1931 (Chandrasekhar to father), Chandrasekhar Archive. There are three letters from Chandrasekhar to Stoner which is in the Stoner Archive.

<sup>16</sup> Letter of 30 April 1931 (Chandrasekhar to father), Chandrasekhar Archive.

been established and is used by astrophysicists such as Eddington and Milne without any overt criticism. Chandrasekhar himself uses this formula and cites Stoner and Anderson in his work although he clearly states that his approach to the problem is from a previously unused polytropic angle.

By the time he sailed for England Chandrasekhar had already acquired some knowledge of quantum mechanics and commenced his original research on the statistical nature of electrons and their behaviour. In his biography of Chandrasekhar, Wali states that at this point, Chandrasekhar had already developed Fowler's application of electron degeneracy in white dwarfs by using Eddington's polytrope to construct a stellar model for white dwarfs finding that the central density within white dwarfs was almost six times the average stellar density. And it was on his journey to England in the summer of 1930, that Chandrasekhar first questioned whether special relativistic effects might come into play at such high densities.<sup>17</sup>

Chandrasekhar was admitted to Cambridge to pursue doctoral research with Fowler as his supervisor in September 1930. And on meeting him, Chandrasekhar immediately presented to Fowler two papers he had completed on his journey, 'The Density of Dwarf Stars', of which Chandrasekhar recalls Fowler saying it was 'quite all right' but that he was going to show it to Milne, and 'The Maximum Mass of Ideal White Dwarfs'.<sup>18</sup> The first paper, in essence, was a reworking of Stoner's non-relativistic formula for a limiting density using Eddington's polytropic theory rather than Stoner's kinetic-gravitational equilibrium as its basis. The second paper introduced the concept of relativistic degeneracy.

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<sup>17</sup> Wali (1991): 76.

<sup>18</sup> Letter of 2 October 1930 (Chandrasekhar to father), Chandrasekhar Archive.

Chandrasekhar's starting point is a degenerate white dwarf star where radiation pressure is taken as zero, thus the main pressure holding the stellar structure against gravitational collapse will be the degenerate gas pressure.

We recall that the total pressure

$$P = p_r + p_g \quad (3.1)$$

where  $p_r$  = radiation pressure

$p_g$  = gas pressure

In this instance radiation pressure is taken to be zero and the star is considered to be an ideal case.

For a fully degenerate gas, the pressure is

$$p_e = (\pi / 60) h^2 / m (3n/\pi)^{5/3} \quad (3.2)$$

where  $n$  is the number of electrons,  $m$  is the mass of the electron and  $h$  is Planck's constant.

Applying the theory of polytropic gas spheres, total pressure then takes the form

$$P = K_1 \rho^{5/3} \quad (3.3)$$

where  $K_1$  is a numerical constant and the ratio of specific heats  $\gamma = 5/3 = 1 + 1/n$  which gives the polytropic index defining the gas sphere to be  $n = 3/2$ .



Chandrasekhar's polytropic approach compared to Stoner's gravitational-kinetic equilibrium approach produces a discrepancy in their results of nearly a factor of two: Chandrasekhar calculates a value for density  $\rho = 2.162 \times 10^6 (M/M_{\odot})^2$  whereas Stoner's value is  $\rho = 3.977 \times 10^6 (M/M_{\odot})^2$ . Their results, however, are of the same magnitude which indicates that this discrepancy may be due to the approximate nature of their calculations rather than a flaw in their theories. The main conclusions that Chandrasekhar drew from his paper were that the radius of a white dwarf was inversely proportional to the cube root of the mass, the density was proportional to the square of the mass and the central density would be six times the mean density.<sup>19</sup> Both Fowler and Milne were impressed with Chandrasekhar's work and the paper was sent to the *Philosophical Magazine* for publication mainly because 'astrophysical papers are not usually published in the Royal Society.'<sup>20</sup>

In a letter to his father dated 22 October 1930, Chandrasekhar writes that he had communicated his result that the maximum mass of a dwarf star is approximately that of our sun to Jeans who replied that he thought Chandrasekhar's result was 'quite important.'<sup>21</sup> Chandrasekhar does not clarify what Jeans' meant by this remark but is trying to convey to his father that there is at least one eminent scientist who is taking his research seriously. But regarding the second paper on the limiting mass, neither Fowler nor Milne found the arguments convincing. Without their encouragement and thus unable to get the support he required to publish his paper in the *MNRAS*, Chandrasekhar sent his short second paper to the *Astrophysical Journal* in Chicago in November.<sup>22</sup> This

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<sup>19</sup> Chandrasekhar (1931a): 595.

<sup>20</sup> Letter of 10 October 1930 (Chandrasekhar to father), Chandrasekhar Archive.

<sup>21</sup> Letter of 22 October 1930 (Chandrasekhar to father), Chandrasekhar Archive.

<sup>22</sup> Wali (1991): 121.

paper contained the relativistic extension of the Fermi-Dirac statistics used in the previous paper.

For a degenerate case where the number of electrons per cubic centimetre is greater than  $6 \times 10^{29}$ , the pressure of the gas will be

$$P = 1/8 (3/\pi)^{1/3} hc n^{4/3} \quad (3.4)$$

where  $h$  is Planck's constant and  $c$  is the velocity of light.

Comparing equation (3.4) to equation (3.3) for ordinarily degenerate electrons, we get the relativistically degenerate polytropic equation for the total pressure

$$P = K_2 \rho^{4/3} \quad (3.5)$$

where  $K_2$  is a numerical constant and the ratio of specific heats  $\gamma = 4/3 = 1 + 1/n$  which gives the polytropic index defining the gas sphere to be  $n = 3$ .

For an ideal case with extreme degeneracy, the upper limit to the mass of an ideal white dwarf would be

$$\begin{aligned} M &= 1.822 \times 10^{33} \\ &= 0.91 M_{\odot}. \end{aligned}$$

where  $M_{\odot}$  is the solar mass.

Chandrasekhar compares this value to Stoner's limiting mass of  $2.2 \times 10^{33}$ , or  $1.09 M_{\odot}$ , concluding that

The 'agreement' between the accurate working out, based on the theory of the polytropes, and the cruder form of the theory is rather surprising in the view of the fact that in the corresponding non-relativistic case the deviations were rather serious.<sup>23</sup>

The factor of nearly 2 that separated their previous results was significantly decreased in the relativistic case.

Chandrasekhar acknowledges Stoner's contribution in the search for a limiting mass, but there is a methodological difference in their investigations and the assumptions that were taken. Chandrasekhar's treatment of the problem is constructed around the standard polytropic model employed by astrophysicists such as Eddington and Milne whereas Stoner employs, what Chandrasekhar and Milne would describe as, a cruder and more approximate method using gravitational contraction as the main source of energy to establish equilibrium. Another important point to come out from these investigations is the problem of density distribution in the stellar models. Chandrasekhar discovered that the density in the interior of the star changes as you proceed outwards with the centre being significantly denser than in the outer envelope. Chandrasekhar's theory here tends towards Milne's theory where the density varies with depth, although Milne's model has clear density demarcations. Stoner himself assumes a uniform density distribution following Eddington's standard model although Stoner began his research from Jeans' liquid star theory.

The main problem that both scientists encountered in getting their work accepted is the approximate nature of their calculations. Stoner's paper had already come under attack by Milne and later by Chandrasekhar who wished to give the problem an exact treatment. Yet, Chandrasekhar's work is also criticised heavily by Milne who prefers an

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<sup>23</sup>Chandrasekhar (1931b): 81-2. See chapter 2 page 21 for Stoner's mass limit.

algebraic treatment rather than just numerical solutions.<sup>24</sup> Recalling these events, Chandrasekhar writes

Soon after my arrival in Cambridge, I gave to R.H. Fowler my two papers in which I gave an account of my results on degenerate stars. In the first of them, I had applied the theory of polytropes to non-relativistic stars. In the first of them, I had applied the theory of polytropes to non-relativistic white dwarfs; and in the second, I had deduced my limiting mass. Fowler forwarded both these papers to Milne. With respect to the first of them, Milne wrote to Fowler that it overlapped with some of his own work and that he (Fowler) communicate it to the *Philosophical Magazine*; but Milne did not seem to think that my second paper was worth publishing in that state, and Fowler did not do anything about it either. Since neither Fowler nor Milne would take steps to publishing this short paper, I sent it to the *Astrophysical Journal* on my own some two months later.<sup>25</sup>

Chandrasekhar finds it curious that neither Fowler nor Milne show any interest in his second paper on the limiting mass believing that it was because ‘neither Fowler nor Milne wanted to accept the fact that there was a maximum mass.’<sup>26</sup> Yet the correspondence between Chandrasekhar and Milne show that Milne was actually very interested in Chandrasekhar's work on relativistic degeneracy and the limiting mass, and he says so encouragingly in his letters to Chandrasekhar. Most of Chandrasekhar's papers that were published in the *MNRAS* were communicated by Milne to the RAS. He was just not convinced of their mathematical and conceptual rigour, and hence their accuracy, and preferred Chandrasekhar to expand his treatment of the subject. Chandrasekhar's results needed to be more exact.

<sup>24</sup> Letter of 29 January 1931 (Milne to Chandrasekhar), Box b427/ folder D4, Milne Archive.

<sup>25</sup> Chandrasekhar (1979), ‘Recollections of E.A. Milne’, Addenda Box 77/ folder 5, Chandrasekhar Archive.

<sup>26</sup> Oral History Archive, Chandrasekhar (1977): 14, 18.

### 3.1.2 Collaboration and Disagreement regarding Milne's Degenerate Core Theory

One morning in October 1930 Milne turned up unannounced to pay Chandrasekhar a visit after receiving Chandrasekhar's paper from Fowler. They talked about degenerate stars and soon began to work together, corresponding frequently, at a rate of once or twice a week from then on. A month after their meeting, Milne delivered at the RAS his paper on the 'Analysis of Stellar Structure' which criticised the methodology behind Eddington's standard stellar model. Chandrasekhar wrote to his father,

the paper is supposed to cause a lot of sensation as Professor Milne and others (including me) believes that he has completely destroyed Eddington's view of the interior of stars.<sup>27</sup>

He explains to his father that his own work on white dwarfs was a limiting case of Milne's more generalised theory. Milne himself considered Chandrasekhar's work to be an extension of his work as, for example, in a letter of 2 November 1930, Milne wrote to Chandrasekhar,

I saw your paper on completely degenerate white dwarfs in their maximum density. Your formulae are particular cases of my analysis of 'collapsed stars', and my formulae  $\rightarrow$  to yours as  $L \rightarrow 0$ .<sup>28</sup>

Milne was based at Oxford since 1928 and was, by this time, working on his degenerate core theory with several research students. In the following two years since their first meeting, Chandrasekhar collaborated with him on several papers linked to Milne's theory which incorporated his work on relativistic degeneracy or, as he called it, relativistic statistics. In this we can see a logical extension, or continuation, of the work he had begun while still a student in India.

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<sup>27</sup> Letter of 14 November 1930 (Chandrasekhar to father), Chandrasekhar Archive.

<sup>28</sup> Letter of 2 November 1930 (Milne to Chandrasekhar), Box b427/ folder D3, Milne Archive. Here  $L$  denotes the stellar luminosity.

Chandrasekhar quickly completes several new papers investigating the dissociation formulae which would determine the statistics to be used when dealing with degenerate and relativistic cases of neutral atoms, ions and electrons. Here Chandrasekhar has to define what is meant by extreme ionisation at very high concentrations when free electrons will still be under the influence of different nuclei, and can be interpreted as being in a state of zero ionisation. The state of the electrons will be determined by the statistics utilised depending on whether they are deemed degenerate or not.<sup>29</sup>

After Milne's paper on stellar analysis was published, Chandrasekhar begins work on extending Milne's theory with a special focus on his collapsed configurations or white dwarfs. Here Chandrasekhar puts forward his theory of limiting mass explaining that the logical extension of the rapid rise in the central density and temperature results in the predominance of the relativistic-mass effect. He applies the equations for relativistically and non-relativistically degenerate states to composite configurations where a relativistically degenerate core is surrounded by a non-relativistically degenerate envelope and a homogeneous core is surrounded by a relativistically degenerate envelope, a homogeneous core being defined as having reached a state beyond which the relativistically degenerate equation of state no longer applies and extreme densities may exist.<sup>30</sup> His investigation leads him to conclude that

the completely relativistic model considered as the limit of the composite series is a point-mass with  $\rho_c = \infty$ !

Chandrasekhar is clearly discussing a singularity resulting as a limit for a completely relativistic star. He continues,

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<sup>29</sup> Chandrasekhar (1931c): 455.

<sup>30</sup> Chandrasekhar (1931d): 464.

The theory gives this result because  $p = K_2 \rho^{4/3}$  allows any density provided the pressure be sufficiently high. We are bound to assume therefore that a stage must come beyond which the equation of state  $p = K_2 \rho^{4/3}$  is not valid, for otherwise we are led to the physically inconceivable result that for  $M = 0.92 \odot \beta^{-3/2}$ ,  $r_1 = 0$ , and  $\rho = \infty$ . As we do not know physically what the next equation of state is that we are to take, we assume for definiteness the equation for the homogeneous incompressible material  $\rho = \rho_{\max}$  where  $\rho_{\max}$  is the maximum density of which matter is capable.<sup>31</sup>

We can see that by 1931, Chandrasekhar has already drawn the conclusion that a singularity will result once the limiting mass is exceeded, as the radius becomes zero and the density infinite. But he is reluctant to concretely draw this ‘physically inconceivable’ conclusion, and is searching for another equation of state into which the star can proceed. Milne's stellar theory itself does not disallow more than two states through which the stellar gas can pass. In fact, he goes as far as to say that if the star loses stability while in a two-phase configuration, then another equation of state must be found to describe the new configuration, although he does not go as far as to describe what that may be.<sup>32</sup>

Milne is impressed by Chandrasekhar's achievements but is cautious saying to Chandrasekhar in a letter of 16 January 1931, ‘your rel. deg. [relativistic degenerate] white dwarf will require careful treatment’<sup>33</sup> and later on 29 January,

I was very much interested in your paper. You have worked out the relativistic degeneracy star most beautifully - I wish other people understood my analysis so completely as you do. ... Where however I must criticise your paper is in its conclusion. You conclude that a dense star cannot have a mass exceeding some value  $M_0$ . But the question then immediately arises: - what is the state of a mass which is very large ( $> M_0$ ) for arbitrarily small  $L$ ?

<sup>31</sup> Chandrasekhar (1931d): 465. Here  $K_2$  is a constant,  $\rho_c$  is the core density,  $r$  is the stellar radius,  $\infty$  is infinity,  $\beta$  is the ratio of radiation pressure to gas pressure and  $\odot$  is the solar mass.

<sup>32</sup> Milne (1932/1936): 19.

<sup>33</sup> Letter of 16 January 1931 (Milne to Chandrasekhar), Box b427/ folder 4, Milne Archive.

Milne points out that the flaw in Chandrasekhar's reasoning is that he cannot prove that solutions appropriate to outer parts of a relativistic degenerate core is Emden's solution  $n = 3$ . This would justify the use of the polytropic equation  $P = K_2 \rho^{4/3}$  to describe a relativistically degenerate star. Milne insists that there could be other solutions. He continues,

Your analysis simply shows that the relativistic equation of state cannot subsist right through to the centre when  $M > M_0$ . Either a new centrally-condensed 'dense' configuration arises, or the new config. requires a fresh internal supporting surface (inside the rel. deg. [relativistic degenerate] core) and a new collapse is foreshadowed. You must investigate this to the bitter end and see what the final state really is. You may be able to prove that such a star must have an incompressible core at the max. density of matter.

He urges Chandrasekhar to show that his solutions and conclusions are not merely due to numerical but also to algebraic, and therefore more general, consequences for the condition of collapse. He ends the letter by saying,

I do hope you will bring the investigation to a conclusion and meet these constructive criticisms of mine. In its present form the conclusion about the masses of the w.d. [white dwarfs] do not hold good, as my ideas go. Why w.d. [white dwarfs] have an upper limit of mass's more likely to depend on the intrinsic physics of the energy-generating forces. ... Your conclusion in its present form arises from the curve's properties of ' $n = 3$ ', but I think you have fallen into the Eddingtonian error of inferring physical consequences from what can be called, an incomplete mathematical treatment.'<sup>34</sup>

To Milne, Chandrasekhar's theory contained too many assumptions and inferences, just like Eddington's.

The main area of research that was attracting the attention of stellar astrophysicists in the early 1930s was the problem of stellar opacity, and Chandrasekhar, responding to Milne's criticisms, soon joins in the race to calculate suitable opacities for the various competing stellar models. The opacity would reveal the interior mechanisms

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<sup>34</sup> Letter of 29 January 1931 (Milne to Chandrasekhar), Box b427/ folder 4, Milne Archive.



of the star which control the rate of energy transfer and even density. Chandrasekhar, together with Kothari and Swirles, discovers during the course of their research that the opacity and absorption within a degenerate electron gas were independent of the gas' density and inversely proportional to the square of the temperature.

Milne's stellar model was the focus of his collaboration with Chandrasekhar since the publication of 'Analysis of Stellar Structure', but their views began to conflict soon afterwards, however, as Chandrasekhar's paper on the limiting mass effectively excluded Milne's criterion for compulsory degeneracy in *all* stars. Although Milne was satisfied with Chandrasekhar's effort and contribution to his theory, Chandrasekhar's focus on white dwarfs and relativistic degeneracy did not bode well for his own stellar theory.

We can see that at this stage Chandrasekhar is more interested in Milne's theory which allows for the possibility of degeneracy in all stars rather than Eddington's standard model which is composed of an ideal gas sphere with degeneracy setting in only in the extreme case of white dwarfs even though he started his investigation with Eddington's polytropic model. Following Milne's lead, in his earlier papers Chandrasekhar is openly critical of Eddington's model and there are several references to mistakes Eddington makes in *Internal Constitution of the Stars*. In fact, Milne's theory is seen by many as a breakthrough in stellar astrophysics that has, until now, been shaped and set by the polytropic models of Eddington and Emden in the past decade. Although these models provide a standard for astrophysicists to work with, they nevertheless seem unsatisfactory due to their ideal and invariant nature. Milne's call for a new methodology and a divergence from the ideal, uniformly dense stellar model is

seen by many younger astrophysicists as being closer to the lines on which real stars are probably structured.

The voluminous correspondence between Milne and Chandrasekhar emphasise their close research collaboration. For example, Milne frequently requests Chandrasekhar to perform calculations on opacity and temperature for a relativistically degenerate gas when they publish papers together. The letters also provide a picture of their relationship from the reserved formality of the first few months where Milne addresses Chandrasekhar as 'Dear Mr. Chandrasekhar' to a more casual 'My Dear Chandrasekhar' as their friendship grows.<sup>35</sup> In fact their relationship seems to be that of mentor and student, changing into friendship as Chandrasekhar proves his technical capability within their field. Upon becoming Fellow of Trinity, Milne asks Chandrasekhar to drop the title of professor in his correspondence explaining,

It used to be a good rule at Trinity that once a man became a Fellow he dropped all titles with other members of the High Table.<sup>36</sup>

In addition to technical matters, Milne advises Chandrasekhar on structuring astronomical articles to tailor them for the *MNRAS*, explaining in one letter,

These are the conventions instilled into me by [ the mathematician Godfrey Harold] Hardy when I first graduated and they are followed by the best stylists.<sup>37</sup>

In the months following his research collaboration with Milne, Chandrasekhar attempts to shape his theory to fit in with Milne's model, often encountering criticism from Milne. He does not give up his ideas regarding relativistic degeneracy, which Milne happily accepts, but he is cautious about concretely emphasising the limiting

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<sup>35</sup> The change in Milne's address towards Chandrasekhar can be seen from the letter of 13 April 1931 (Milne to Chandrasekhar), Box b427/folders D 6/7, Milne Archive.

<sup>36</sup> Letter of 20 December 1933 (Milne to Chandrasekhar), Box b427/folder D18, Milne Archive.

<sup>37</sup> Letter of 9 December 1931 (Milne to Chandrasekhar), Box b427/folder D9, Milne Archive.

mass which would invalidate Milne's theory. But in his paper, 'Some Remarks on the State of Matter in the Interior of Stars', published in 1932, Chandrasekhar argues,

for all centrally condensed stars of mass greater than  $M$ , the perfect gas equation of state does not break down, however high the density may become, and the matter does not become degenerate. An appeal to the Fermi-Dirac statistics to avoid the central singularity cannot be made.<sup>38</sup>

The critical parameter he uses to produce this limiting mass is  $\beta$  the ratio of radiation pressure to gas pressure. He breaks away from Milne's model by implying that Milne had overlooked the importance of a limiting mass beyond which degeneracy does not set in, and hence the Fermi-Dirac statistics cannot be used to find the distribution of electrons in the stellar gas, if radiation pressure is greater than a tenth of the total pressure. By this stage, Chandrasekhar has accepted that once the limiting mass is exceeded, the radius of that star will tend to zero and leave a singularity, concluding,

great progress in the analysis of stellar structure is not possible before we can answer the following fundamental question: *Given an enclosure containing electrons and atomic nuclei, (total charge zero) what happens if we go on compressing the material indefinitely?*<sup>39</sup>

This is the important, but perplexing, question Chandrasekhar asks at the end of his exposition of his theory, and it is the question which will torment Eddington and other astrophysicists who had, up to this point, assumed that all stars end their lives as stable white dwarfs, not as badly behaved stars.

Chandrasekhar is drawn into the Eddington-Milne controversy through his work on opacity and Milne's stellar theory. The main contesting point between Eddington and Milne is that Eddington only employs one equation of state throughout his stellar model, that of a perfect gas,

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<sup>38</sup> Chandrasekhar (1932): 324.

$$P = (k/m_H\mu)\rho T$$

whereas Milne employs both the perfect gas and the degenerate gas equations of state,

$$P = K_1\rho^{5/3}$$

Except in the extreme case of white dwarfs, Eddington uses only the perfect gas equation, whereas Milne employs both equations for his standard stellar models. In his work, Chandrasekhar identifies another equation of state, that of a relativistic degenerate gas, particularly in certain cases of white dwarfs,

$$P = K_2\rho^{4/3}$$

This equation replaces the ordinary degenerate gas equation at extremely high densities when the effects of special relativity become dominant and relativistic corrections are added.

The paper, ‘Some Remarks on the State of Matter in the Interior of Stars’, was published while Chandrasekhar was spending part of his final year as a Cambridge research student at Copenhagen from August 1932 to May 1933. Due to its unusual, and radical, stance regarding the established astrophysical theories, the paper went through several criticisms and rewriting before it was finally published in *Zeitschrift für Astrophysik*. Chandrasekhar later writes of the circumstances surrounding the publication of this paper which had originally been written in 1931,

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<sup>39</sup> Chandrasekhar (1932): 327. The emphasis is Chandrasekhar's.

I withheld publication because of E.A. Milne's dissent. A year later, when I joined the Institut For Teoretisk Fysik in Copenhagen, I discussed the paper with Leon Rosenfeld, who strongly urged me to publish it. To avoid additional discussions with Milne, I sent it to the *Zeitschrift für Astrophysik*, with editorial offices at the Astrophysikalisches Observatorium in Potsdam. But Professor Milne happened to be visiting Potsdam at that time and he was asked to referee the paper; and in a letter directly written to me, he stated, 'Unfortunately I have been unable to recommend acceptance, as the paper contains a mistake in principle, and in any case it would only do your reputation harm if it were published.'<sup>40</sup>

Milne was unhappy with Chandrasekhar's treatment of the thermodynamic equations Chandrasekhar had used when discussing discontinuity of phase. He believes that the thermodynamic potentials of the two phases must be equal and Chandrasekhar's treatment would cause any stellar configuration using these equations to become 'violently unstable' and ultimately 'catastrophic'.<sup>41</sup>

The misunderstanding was soon cleared, however, once Chandrasekhar persuaded Milne to accept the validity of his theory and, having made a few superficial changes, Chandrasekhar writes to his father on 21 October 1932,

after a longdrawn out controversy, Milne has accepted my results and says 'it is indeed a most important theory you establish.'<sup>42</sup>

We can say that by this stage, Chandrasekhar's conceptual understanding of his theory is complete. He began to work on the Fermi-Dirac statistics while a student in India and discovered the limiting mass on his voyage to England in 1930. He was convinced of its significance but did not understand what its implications were. By the time he completes the final draft for his paper for *Zeitschrift für Astrophysik* in 1932, Chandrasekhar says, 'I was completely clear as to what the situation was at the time.'<sup>43</sup>

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<sup>40</sup> Chandrasekhar (1989): xii; Also in Chandrasekhar (1979), 'Recollections of E.A. Milne', Addenda Box 77/ folder 5, Chandrasekhar Archive.

<sup>41</sup> Letter of 1 October 1932 (Milne to Chandrasekhar), Box b427/ folder D14, Milne Archive.

<sup>42</sup> Letter of 21 October 1932 (Chandrasekhar to father), Box 3/ folder 5, Chandrasekhar Archive.

<sup>43</sup> Oral History Archive, Chandrasekhar (1977): 14.

It is an interesting coincidence that Chandrasekhar's two most controversial papers on the limiting mass which were published during this period were published in foreign journals, the *Astrophysical Journal* in Chicago and *Zeitschrift für Astrophysik* in Potsdam. The British astronomers did not actively prevent the publication of his papers; they were simply not interested in publishing them.<sup>44</sup> An additional factor is that the referee would most likely have been Milne who would not have recommended them for publication. Eddington, who had welcomed and helped Stoner with his contribution on the limiting mass of white dwarfs, may also have been a suitable candidate for refereeing Chandrasekhar's paper, but we must remember that at this stage, Chandrasekhar did not have any contact with Eddington, only Fowler and, through him, Milne.

During this period Chandrasekhar ceases to work on relativistically degenerate stars and begins to construct his doctoral thesis on distorted polytropes which he completes in the summer of 1933. After his PhD *viva voce* where he was examined by Eddington and Fowler, and which he subsequently passed, he sat for his Fellowship examination at Trinity College in August. He became Fellow in October 1933, and his future for the next three years was secured at Cambridge. In a congratulatory letter to Chandrasekhar, Milne writes,

It is a further satisfaction to me to feel that the elector's decision confirms my own judgement. I was called in as referee and wrote a long careful critical account of your paper, not always agreeing with them, but concluding that they showed a tremendous increase of power and maturity as the investigations mounted up, and stating my considered opinion at the end that your work was most definitely of fellowship standard and would in the future be of still more importance to science. ... Of course you mustn't think that I am claiming any part in your election - I thought you merely might be interested in the above piece of confidential information. It was the

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<sup>44</sup> Chandrasekhar (1931b); (1932).

intrinsic value of your contributions that has brought you your reward.<sup>45</sup>

There is no doubt that Milne respected Chandrasekhar's academic abilities. Their close collaboration and correspondence certainly places Milne in a position where he probably was the most appropriate person to judge Chandrasekhar in Britain regarding his astrophysical work, and he does not find Chandrasekhar lacking. And Milne is not the only one. When Chandrasekhar had applied for a place at Cambridge in 1930, his success was ensured by Fowler's support of which Chandrasekhar recalls Winstanley, the Senior Tutor at Trinity, as saying 'he knew Fowler for the last 13 years, and that Fowler rarely recommends.'<sup>46</sup>

### 3.1.3 Rethinking the Limiting Mass and Establishing an Exact Theory

Chandrasekhar had placed his work on the limiting mass aside for over a year so that he could work on opacity and temperature calculations with Milne and to complete his PhD thesis. So it was not until the autumn of 1934 after his return from a month-long visit to Russia that Chandrasekhar immerses himself in his stellar structure research again.

By the end of the year, he writes,

I have sent an advance 'statement' of my general results in stellar structure as a short article to the 'Observatory'. My work in this field has been developing in a very fascinating way.<sup>47</sup>

He has had other successes as well, his work on stellar atmosphere is to be published as a volume in the *Cambridge Mathematical Tracts* series where

<sup>45</sup> Letter of 10 October 1933 (Milne to Chandrasekhar), Box b427/ folder D18, Milne Archive.

<sup>46</sup> Letter of 3 September 1930 (Chandrasekhar to father), Box 3/folder 2, Chandrasekhar Archive.

<sup>47</sup> Letter of 28 October 1934 (Chandrasekhar to father), Box 3/ folder 8, Chandrasekhar Archive.

one has the satisfaction of 'registering' oneself as a serious Cambridge mathematician - almost all the eminent Cambridge mathematicians have written in this series.<sup>48</sup>

Chandrasekhar is also commencing a series of twenty lectures at the beginning of 1935 on 'Special Problems in Astrophysics' where he lectures three times a week for a salary of £10. He writes, elated, to his father,

I am the first Indian to have given University lectures at Cambridge - I mean no other had had the opportunity.<sup>49</sup>

Chandrasekhar explains, in Wali's biography, that the reason he returned to his work on the limiting mass was due to the encouragement he had received from the Russian astrophysicist Victor Ambartsumian. On hearing one of Chandrasekhar's lectures at the Pulkova Observatory in Leningrad about Chandrasekhar's work on the limiting mass, Ambartsumian had suggested that Chandrasekhar should cut down the number of approximations and attempt to construct an exact theory.<sup>50</sup> There is no mention, however, of any interesting interaction or collaboration with Landau while Chandrasekhar was visiting Russia.

It would seem from the papers that Chandrasekhar had published during his time at Cambridge that, although he took a break from his work on the limiting mass to write his thesis, he never gave up his ideas through lack of any deep interest from his colleagues. Indeed, it would seem that he made several attempts to incorporate his conclusions into the papers he collaborated with Milne on his two phase stellar theory. The following two passages from a paper Chandrasekhar published in March 1934, before his trip to Russia show clearly that by this stage, before he had begun any exact calculations on the limiting mass, he was already beginning to reject Milne's compulsory

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<sup>48</sup> Letter of 16 November 1934 (Chandrasekhar to father), Box 3/ folder 8, Chandrasekhar Archive.

<sup>49</sup> Letter of 15 February 1935 (Chandrasekhar to father), Box 3/ folder 9, Chandrasekhar Archive.

<sup>50</sup> Wali (1991): 117.



degenerate stellar models and favour Eddington's standard model. He agrees with Eddington that the lack of knowledge regarding stellar energy sources does not impede any investigation of stellar structure, in contradiction to Milne's criticism that stellar astrophysics is in no way complete without this knowledge. Thus Chandrasekhar writes,

It is necessary, however, to state explicitly that lack of complete information regarding the internal distribution of energy sources is not very serious when we are primarily concerned with the hydrostatic equilibrium of the star.<sup>51</sup>

And concludes,

The general evidence then is in favour of Eddington's perfect gas hypothesis for ordinary stars, and it would follow that the physical conditions in the interior of stars derived by him should be near the truth.<sup>52</sup>

Until the publication of this paper, Chandrasekhar has followed Milne's lead in criticising Eddington. Yet here, he has turned the tables and although he does not explicitly criticise Milne he does show that Milne's theory does not hold true according to the presence of a limiting mass and that Eddington's model is the more accurate one. His actions do not seem as clear cut as Wali attempts to show in his biography: there is no instant repudiation of his earlier work, where Chandrasekhar leaves his theory and then picks it up again after returning from Russia. He has steadily incorporated his theory into the controversy between Eddington and Milne, using the theories of the two astrophysicists as controls by which he can arbitrate between the differing models. Chandrasekhar is certainly aware that he is entering a controversial debate as we can see from the following recollection,

In 1934, when I was working on my exact theory of the white dwarfs, I became extremely confident that my work would be recognized while resolving at the same time the controversy between Eddington and Milne. And since I did not want to have any public controversy with

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<sup>51</sup> Chandrasekhar (1934a): 93.

<sup>52</sup> Chandrasekhar (1934a): 98.

Milne, I had him visit me in Cambridge so that I would have a chance to explain to him the results of my own work and how it essentially destroyed the basis of his (Milne's) own ideas with respect to the degenerate cores of stars.<sup>53</sup>

Why did Chandrasekhar choose to side with Eddington when he had criticised Eddington's theory several times in his earlier papers? It is only in 1934 that he mentions Eddington's theory in a positive manner. Chandrasekhar's discovery, that beyond a certain limiting mass degeneracy would not set in, reverts his stellar model back into a perfect gas phase. Thus stars with masses greater than this limit would continue to function as perfect gas spheres following Eddington's standard model. Eddington's theory has not been refuted as Milne claimed; it was Milne's own theory that had to be modified.

The situation can be summarised in the following way. Eddington's standard model was the established theory pertaining to describe stellar structure. Milne's theory, which was put forward several years later, attempted to revolutionise theoretical astrophysics by confronting Eddington's theory which had assumed a dogmatic position which, Milne believed, impeded astrophysical research through its questionable methodology and speculative conclusions. Chandrasekhar, who had studied the rudimentary theories of astrophysics using Eddington's *Internal Constitution of the Stars*, becomes fascinated with Milne's theory, especially the degeneracy angle, which is pertinent to his immediate research on Fermi-Dirac statistics, which is further encouraged by his friendship and collaboration with Milne. Soon, however, Chandrasekhar's limiting mass shows that degeneracy cannot set in for stars more massive than this limit and Milne's theory is refuted. On the other hand, this now shows

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<sup>53</sup> Chandrasekhar (1979), 'Recollections of E.A. Milne', Addenda Box 77/ folder 5, Chandrasekhar Archive.

that Eddington's theory is valid for massive stars. Chandrasekhar finds that his theory, in fact, supports Eddington's theory and promptly switches sides. In a high profile controversy such as that between Eddington and Milne, Chandrasekhar aimed to show that his limiting mass could be the decisive factor in determining who would win.

Chandrasekhar identifies three equations for pressure, and depending on the value of the pressure as it increases, the stellar model will progress from a perfect gas state to an ordinarily degenerate and ultimately to a relativistically degenerate state.

$$p_{\text{perfect}} = (k/m_H\mu) \rho T \text{ for a perfect gas,}$$

$$p_{\text{ord.deg.}} = 1/20 (3/\pi)^{2/3} h^2 / m(m_H\mu)^{5/3} \rho^{5/3} = K_1 \rho^{5/3} \text{ for an ordinarily degenerate gas and}$$

$$p_{\text{rel.deg.}} = (3/\pi)^{1/3} hc / 8(m_H\mu)^{4/3} \rho^{4/3} = K_2 \rho^{4/3} \text{ for a relativistically degenerate gas.}$$

Thus a star will be relativistically degenerate if, and only if

$$p_{\text{rel.deg.}} > p_{\text{ord.deg.}} > p_{\text{perfect.}}$$

Chandrasekhar's first official announcement regarding his exact theory of limiting mass was published in the November issue of the *Observatory*. It is in this paper that Chandrasekhar specifically states his conclusion which would later cause great discomfort for Eddington,

Finally, it is necessary to emphasize one major result of the whole investigation, namely, that it must be taken as well established that the life-history of a star of small mass must be essentially different from the life-history of a star of large mass. For a star of small mass the natural white-dwarf stage is an initial step towards complete extinction. A star of large mass ... cannot pass into the white-dwarf stage, and one is left speculating on other possibilities.<sup>54</sup>

What the other possibilities might be, however, Chandrasekhar does not say. But this is the first time that the critical nature of the stellar mass is signified in print. Chandrasekhar specifically uses the term mass, rather than temperature, density or opacity, to distinguish the particular state on which a star may find itself dependant at the end of its evolution.

However, in a paper written nine months earlier, Chandrasekhar does write,

When  $M \rightarrow 5.736\mu^{-2} \times \odot\beta^{-3/2}$  the star tends to contract to a point. Hence, by taking the mass sufficiently near this limit we can obtain arbitrarily high values for the central density; but it is very doubtful if this result has any particular significance.<sup>55</sup>

It would seem that he was well aware of what a limiting mass will entail, as in his earlier papers, but not its significance.

### 3.2 The Exact Theory of the Limiting Mass for White Dwarfs

We have seen Chandrasekhar nursing his theory through his doctoral years by first trying to assimilate it to Milne's theory. But upon discovering that they were incompatible, and facing objections from Milne, he began to look towards Eddington's standard model for support. After the successful outcome of his Fellowship examinations Chandrasekhar chose to renew his attempt at completing his theory, this time from a point directly between that of Eddington and Milne's stellar models placing

<sup>54</sup> Chandrasekhar (1934b): 377.

<sup>55</sup> Chandrasekhar (1934a): 97.  $M$  is the stellar mass,  $\mu$  is the mean molecular weight,  $\odot$  is the solar mass and  $\beta$  is the ratio of the gas pressure to the total pressure.

him in the centre of their controversy. Throughout the Eddington-Milne controversy which raged during Chandrasekhar's doctoral years, Chandrasekhar had found himself siding with Milne, mainly because of Milne's research in the area of degenerate stellar material. Apart from publishing a couple of papers on the upper limits to stellar densities with the aid of Stoner's calculations, Eddington had not been actively working on his stellar theory during this period except to defend himself against Milne's attacks.

Upon his return from Russia Chandrasekhar began to make exact numerical calculations towards a value for his limiting mass, ridding himself of any approximations that he had previously utilised in his research. He spent the months leading up to the January meeting of the RAS tirelessly working towards a complete theory, and in doing so obtained Eddington's encouragement because the limiting mass would indicate that Eddington's model would triumph over that of Milne's. Recalling that period of intense research, Chandrasekhar tells Wali,

I was very pleased ... because Eddington seemed to understand that. He took a great deal of interest in the day-to-day progress of my work. He even got me the only hand calculator, a Brunsviga, that was around and was being used by Gunnar Sttenhold, a Norwegian visitor. Sttenhold was not happy of course. During the three months from October through December, Eddington came to my rooms quite often, at least once, sometime twice or three times, a week. As my numerical work progressed, I would show him the point on the emerging graph.<sup>56</sup>

Lending him his treasured Brunsviga calculator, which he had already promised to another, and checking Chandrasekhar's results several times a week for three months, it would seem that Eddington was seriously interested in Chandrasekhar's calculations. In an interview for the Oral History Archive at the Institute of Physics in 1977 Chandrasekhar describes the events leading up to his controversy with Eddington in detail. The story is echoed in Wali's biography and there are no discrepancies in any

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<sup>56</sup> Wali (1991): 123.

archival and published accounts.<sup>57</sup> The account he gave of the controversy with Eddington to his colleagues and friends have been consistent, but sparse, and Chandrasekhar does not offer any reasons or explanations as to why the controversy occurred in the first place and why Eddington acted in the way that he did.

After three months of intense research, Chandrasekhar presented his findings at the January meeting of the RAS in 1935. He had received the programme in the evening prior to the meeting from the assistant secretary of the RAS and was surprised to see that Eddington was going to talk, of all things, on relativistic degeneracy immediately after him. Chandrasekhar recalls

I was really annoyed, because here was Eddington coming and talking to me, week after week, about my work while he was writing a paper himself and he never told me about it. ... and talking to me all the time about the work. And I was telling him, 'How can a star evolve? massive stars must behave differently,' and so on -- all this was being talked about.<sup>58</sup>

Eddington refused to reveal the content of his talk after dinner that evening, merely saying that he had asked the secretary to grant Chandrasekhar a fifteen minute extension for his talk, and when confronted by McCrea before the meeting, had said to Chandrasekhar, 'That's a surprise for you.'<sup>59</sup>

### 3.2.1 The Papers

Chandrasekhar's talk, which was the culmination of his relativistic degeneracy research which began on his journey to Cambridge and incorporated his exact solutions and numerical calculations, was published as two lengthy papers in the January 1935

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<sup>57</sup> Chandrasekhar (possibly 1972), 'Historical notes on some astrophysical problems', p.13-22, Box1/ folder 1, Chandrasekhar Archive; Israel (1987): 212-222; Tayler (1996); Thorne (1992): Chp. 4; Wali (1991): Chp. 5.

<sup>58</sup> Oral History Archive, Chandrasekhar (1977): 33.

<sup>59</sup> Oral History Archive, Chandrasekhar (1977): 34.

issue of the *MNRAS*. They were followed by Eddington's offensive against relativistic degeneracy and a discussion in which there is a contribution by Milne. The summary of Chandrasekhar's talk states,

Dr. Chandrasekhar read a paper describing the research which he has recently carried out ... investigating the equilibrium of stellar configurations with degenerate cores. He takes the equation of state for degenerate matter in its exact form that is to say, taking account of relativistic degeneracy. An important result of the work is that the life history of a star of small mass must be essentially different from that of a star of large mass. There exists a certain critical mass  $M$ . If the star's mass is greater than  $M$  the star cannot have a degenerate core, but if the star's mass is less than  $M$  it will tend, at the end of its life history towards a completely collapsed state.<sup>60</sup>

The talk filled out the preliminary sketch of Chandrasekhar's research project that he had outlined in his short paper of November 1934 in *Observatory*. He supports his theory with numerical tables and several graphs which show the calculated values for density, mass, radii and other parameters particular to varying stellar configurations. Chandrasekhar no longer avoids the problem of stellar radii tending to zero as the limiting mass is approached as this would be taken as a 'natural limit.'<sup>61</sup>

He establishes from his theory a number of points which distinguish between the stellar theories of Eddington and Milne. First he makes clear that there are three different equilibrium states which can describe a star: the perfect gas, the ordinarily degenerate and the relativistically degenerate states. Using the standard polytropic model as a base, Chandrasekhar shows that the solutions to Emden's polytropic equations with Emden functions of  $n = 3/2$  and  $n = 3$  describe the ordinarily degenerate and relativistically degenerate states respectively.

Chandrasekhar's theory hinges on the solution of one exact differential equation of the form

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<sup>60</sup> Proceedings of the RAS meetings (1935): 37.

$$1/\eta^2 d/d\eta (\eta^2 d\phi/d\eta) = -(\phi^2 - 1/y_0^2)^{3/2}$$

which is similar to the form of the Emden equation for polytropes on which Eddington's standard model is based.

The final point which underlies Chandrasekhar's theory is the insignificant influence of radiation pressure on the overall gas pressure within the star as it approaches degeneracy. The ratio of the radiation pressure to the gas pressure is taken as unity or  $\beta = 1$ .<sup>62</sup> Chandrasekhar shows that the effects of radiation pressure decrease due to a decrease in luminosity and also as conductivity increases due to extreme ionisation at higher densities. This is a move away from Eddington's model which is a perfect gas sphere where the effect of radiation pressure is comparable to that of ordinary gas pressure. It is only under extreme density, in the case of white dwarfs, that Eddington reverts to electron degeneracy pressure.

Chandrasekhar's investigation shows that each mass has a density distribution that is unique to itself unlike Eddington's polytropic model which has uniform density.<sup>63</sup> One of the assumptions that are incorporated in the theory is that for degeneracy to set in, at some point, deviations from the perfect gas law occur. Therefore one of the questions that must be answered is whether the formulae which Chandrasekhar uses predict such deviations.<sup>64</sup> Chandrasekhar had already established in a paper of 1932 that if the radiation pressure is greater than a tenth of the whole pressure, degeneracy will not set in due to the rapid increase of temperature, and Eddington's model must be used to

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<sup>61</sup> Chandrasekhar (1935a): 207 (footnote).

<sup>62</sup> Chandrasekhar (1935a): 208.

<sup>63</sup> Chandrasekhar (1935a): 214.

<sup>64</sup> Chandrasekhar (1935b): 226.



describe the star.<sup>65</sup> Therefore using his research from earlier papers, he explores more fully the conditions leading to the development of degeneracy in the stellar core and the choice of equation of state (perfect gas or degenerate) which must be used to describe the stellar gas. His previous conclusions regarding the existence of a limiting mass and the development of relativistic degeneracy at high densities are accepted without question and are not scrutinised. What Chandrasekhar has tried to do is to shape his theory into an acceptable algebraic and numerical form with complete derivations and calculations and in doing so shed any unconvincing and untenable assumptions.

He concludes,

the fundamental distinction made on the standard model between masses less than and greater than  $M$  - in that for  $M > M$  the equilibrium configurations are necessarily wholly gaseous (in the perfect gas sense) - is only a counterpart on the standard model of a more general result that all stars of sufficiently large mass are necessarily perfect gas configurations; for the increased dominance of radiation pressure for large stellar masses is quite a general result and, as we have already seen, the possibility of degeneracy is entirely excluded if only the radiation pressure is greater than a tenth of the total pressure throughout the entire mass. ... the result will always be that stars of mass greater than a certain limit will all be perfect gas configurations and will therefore conform to Eddington's mass-luminosity relation however much they may contract. As a result such stars can never pass *directly* into the white-dwarf stage.<sup>66</sup>

We can see that Chandrasekhar finally arrives at Eddington's model rather than Milne's by explicitly stating that stars with mass greater than the limit will not be degenerate. Yet his discussion of ordinary and relativistic degeneracy does not conform to Eddington's perfect gas model. Chandrasekhar is still in the middle of the Eddington-Milne controversy, not exclusively opting for either, but his results show that compared to Eddington's model, Milne's is definitely inaccurate because one of the basic tenets of the theory, that of a compulsory degenerate core, is shown to be wrong.

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<sup>65</sup> Chandrasekhar (1935b): 226; (1932): 321; (1934a): 95.

Chandrasekhar's conclusion that massive stars cannot pass directly into the white dwarf stage opens a new avenue of research, that of supernova phenomena. This is used to explain another route for a star to become a white dwarf, through the ejection of excess matter which would decrease the stellar mass so that degeneracy can set in. He explains this is possible only for a small range of masses in contrast to Milne who believes that any star of large mass can undergo a 'nova outburst'.<sup>67</sup>

### 3.3 Eddington's Attack: On 'Relativistic Degeneracy'

Immediately after Chandrasekhar had delivered his talk, Eddington is invited to present his paper on 'Relativistic Degeneracy.' He begins,

Dr. Chandrasekhar has been referring to degeneracy. There are two expressions commonly used in this connexion, 'ordinary' degeneracy and 'relativistic' degeneracy, and perhaps I had better begin by explaining the difference.<sup>68</sup>

Eddington proceeds to explain that the formula connecting electron pressure and density,  $P_e = K\rho^{5/3}$  for ordinary degeneracy changes at higher densities to a 'more complicated relativistic formula' or  $P_e = K\rho^{4/3}$  where the density tends from  $\rho^{5/3} \rightarrow \rho^{4/3}$ .<sup>69</sup> It would seem as though by using the phrase 'more complicated' Eddington is implying that by using special relativity, Chandrasekhar has produced an *unnecessarily* complicated theory. We can see that Eddington's argument is with the concept rather than the mathematics of Chandrasekhar's reasoning. He continues by stating that Chandrasekhar's relativistic degeneracy formula 'has defeated the original intention of Prof. R.H. Fowler who first applied the theory of degeneracy to astrophysics.' Fowler had solved Eddington's paradox using the newly discovered Fermi-Dirac statistics in

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<sup>66</sup> Chandrasekhar (1935b): 257.

<sup>67</sup> Chandrasekhar (1935b): 257-8.

<sup>68</sup> Eddington (1935a): 37.

1926 to introduce electron degeneracy pressure which would stabilise the white dwarf star against gravitational collapse. But Chandrasekhar's limiting mass which uses 'the relativistic formula which has been accepted for the last five years'<sup>70</sup> has revived the problem once again, and 'when its supply of subatomic energy is exhausted, the star must continue radiating energy and therefore contracting - presumably until, at a diameter of a few kilometres, its gravitation becomes strong enough to prevent the escape of radiation.'<sup>71</sup> Here, Eddington is clearly describing a singularity. He dismisses this notion immediately and continues,

Dr. Chandrasekhar had got this result before, but he has rubbed it in in his last paper; and, when discussing it with him, I felt driven to the conclusion that this was almost a *reductio ad absurdum* of the relativistic degeneracy formula. Various accidents may intervene to save the star, but I want more protection than that. I think there should be a law of Nature to prevent a star from behaving in this absurd way.<sup>72</sup>

Eddington begins his talk by announcing that relativistic degeneracy does not exist. But he later admits that the relativistic formula has been used for the past five years and that he is unhappy with Chandrasekhar's result which he believes makes a mockery of the formula. It is unclear whether Eddington could not accept the complete concept of relativistic degeneracy, or whether the concept was unacceptable, but the mathematical formula itself, for practical purposes, was deemed sound. In an interview in November 1996, McCrea explained that Eddington was not opposed to relativistic degeneracy itself, but to Chandrasekhar's use of relativistic degeneracy in the case of white dwarfs.<sup>73</sup> Although McCrea believes this to be the case, Eddington's diatribe

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<sup>69</sup> In this paper Eddington uses the symbol  $\sigma$  instead of the usual  $\rho$  to denote density.

<sup>70</sup> Eddington (1935a): 38.

<sup>71</sup> Eddington (1935b): 195. The term black hole was not *seriously* discussed until the 1960s when it was first coined by John Archibald Wheeler.

<sup>72</sup> Eddington (1935a): 38.

<sup>73</sup> Interview with McCrea (8 November 1996).

against relativistic degeneracy in all of his papers and books since the January 1935 RAS meeting would indicate otherwise. Eddington acknowledges that there is no problem regarding

the mathematical derivation behind the relativistic degeneracy formula as given in astronomical papers ... One has to look deeper into its physical foundations, and these are not above suspicion.<sup>74</sup>

And, later in another paper, that 'its physical foundation does not inspire confidence'.<sup>75</sup> But the rejection of the concept of relativistic degeneracy would surely imply the rejection of the formula. What Eddington did not have a problem with is the *mathematical* derivation: there was no error in Chandrasekhar's mathematical treatment of the formula. What Eddington takes offence at is Chandrasekhar's use of relativistic degeneracy in the first place.

And here we come to the crux of Eddington's argument,

The formula is based on a combination of relativity mechanics and non-relativity quantum theory, and I do not regard the offspring of such a union as born in lawful wedlock. I feel satisfied myself that the current formula is based on a partial relativity theory, and that if the theory is made complete the relativity corrections are compensated, so that we come back to the 'ordinary' formula.<sup>76</sup>

By the 'ordinary' formula Eddington is referring to Fowler's formula for ordinary electron degeneracy where pressure varies as  $\rho^{5/3}$  rather than  $\rho^{4/3}$  and no relativistic corrections are needed.

The argument he uses refers to the incompatibility of non-relativistic quantum mechanics and special relativity, or the 'unholy alliance',<sup>77</sup> which together forms the relativistic degeneracy formula used by Chandrasekhar, Stoner and Anderson. To illustrate this incompatibility, he uses a wave analogy to describe the motion of

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<sup>74</sup> Eddington (1935a): 38.

<sup>75</sup> Eddington (1935b): 194.

<sup>76</sup> Eddington (1935a): 38; (1935b): 195.

electrons in a perfect gas and ordinarily degenerate states. Eddington describes the motion of a perfect gas electron as that of a progressive wave travelling about in all directions whereas he describes a degenerate electron as a standing wave. He explains that

an electron represented by a standing wave is a quite different sort of entity from the electron represented by a progressive wave. The former is constantly changing its identity. ... The electron represented by a progressive wave can be brought to rest by a Lorentz transformation and it then becomes a standing wave. This transformation introduces a factor into the equation, which is not needed if the waves referred to are standing waves originally. My main point is that the Exclusion Principle presupposes analysis into standing waves, and this has been wrongly combined with formulae which refer to progressive waves.<sup>78</sup>

Eddington argues that Chandrasekhar had tried to combine together a relativistic formula for a non-degenerate electron together with a non-relativistic formula for a degenerate electron, thinking that they refer to the same electron state. Eddington disagrees saying they refer to two completely different electron states coming from opposing relativistic and quantum mechanical positions and therefore require different formulae which cannot be used together, and states

The misunderstanding has arisen through applying the name electron to two quite different products of wave analysis. ... The division of phase space into cells is effected entirely by standing waves. There is no such partition associated with progressive waves, which always correspond to a continuous series of solutions. It is therefore a fallacy to use the momentum vector of a progressive wave system in conjunction with the cell division of a standing wave system.<sup>79</sup>

Eddington explains away the need for relativistic corrections by adding that in quantum mechanical terms, the 'change of mass with velocity arises because it is necessary to rotate the axes of space and time in order to obtain a standing wave.' If the electron is already a standing wave, there would be no need for this rotation and, hence, no need for

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<sup>77</sup> Eddington (1935*b*): 195.

<sup>78</sup> Eddington (1935*a*): 39.

a relativistic correction.<sup>80</sup> This is what, Eddington believes, has confused Chandrasekhar.

Eddington published a more substantial paper, ‘On “Relativistic Degeneracy”’, reiterating his opposition in the *MNRAS* with detailed explanations of his quantum mechanical arguments, mainly for the momentum vector which Chandrasekhar, in his paper, had shown approaches  $mc$ , the relativistic momentum. Eddington's opposition, in both his talk and paper, rests on two things: his refusal to accept the consequence of relativistic degeneracy which will plunge white dwarfs back into a state of uncertainty, undoing Fowler's work with ordinary electron degeneracy, and his refusal to accept what he believes is an unconvincing combination of non-relativistic quantum mechanics and special relativity.

Eddington continues his argument against the relativistic degeneracy formula ‘which has been widely but uncritically accepted’ in *Relativity Theory of Protons and Electrons* which was published the following year.<sup>81</sup> Here again he discusses the confusion caused by the standing (denoting the steady state distribution) and progressive waves to describe the electrons which are restricted by the Pauli Exclusion Principle. Eddington's view is that in normal equilibrium, kinetic and potential energies of the electrons are equal. At constant density, energy is wholly potential at absolute zero (complete degeneracy), and wholly kinetic at high temperatures. Eddington does not distinguish between electrons and protons insisting that at high temperatures the distribution given by the Fermi-Dirac statistics may be correct, but at low temperatures, if particles still have kinetic energy, if the density is high enough this would mean that

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<sup>79</sup> Eddington (1935*b*): 203.

<sup>80</sup> Eddington (1935*b*): 204.

<sup>81</sup> Eddington (1936): 253. This book is generally thought to be the precursor to Eddington's *Fundamental Theory*.

even protons will have kinetic energy thus contradicting the laws of thermodynamics.

By introducing degeneracy, one is introducing progressive waves into the problem and

hence relativistic transformations of the coordinates.<sup>82</sup> Eddington continues,

there is no reason to suppose that in the degenerate state the electrons have higher speeds than the protons ... Investigators seem to have been misled by trusting to the classical picture of moving particles. When a particle is forced up into a state of high energy by the occupation of the lower states, the current theory pictures the energy in the classical way as translation with high velocity. It accept this so literally that it supposes that the particle could be reduced to rest by a Lorentz transformation, and thereby calculates its change of mass with velocity. But non-transferable energy cannot logically be represented that way.<sup>83</sup>

Eddington also has a problem with the indistinguishability of particles inherent in

Fermi-Dirac statistics where it is impossible to track any individual particle.

Eddington's understanding of the Pauli Exclusion Principle is extremely confusing

and colours his explanations regarding standing and progressive waves. He sees

standing waves as describing a composite gas and progressive waves as describing

individual particles. Standing waves describe a steady state in which the Pauli

Exclusion Principle may apply. Yet he refers to physicists assuming that the wave

function does not have to be discontinuous and using progressive waves to

describe a gas in which the Pauli Exclusion Principle applies. Eddington is

perplexed that physicists can change from one to the other without justification

and therefore rejects the exclusion principle and Fermi-Dirac statistics.<sup>84</sup> But

Eddington himself does the same. In his two previous papers on relativistic

degeneracy, he argued that standing waves represent degenerate electrons, yet in

*Relativity Theory of Protons and Electrons*, it is the progressive waves which

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<sup>82</sup> Eddington (1936): 246-247.

<sup>83</sup> Eddington (1936): 254.

<sup>84</sup> Eddington (1936): 233; Eddington (1946): 87-91.

represent them.<sup>85</sup> And as we recall, Eddington's earlier reference to Fermi-Dirac statistics as Bose-Einstein statistics clearly show that Eddington did not fully comprehend quantum mechanics.<sup>86</sup>

Towards the end of the argument, Eddington tries to show that the Coulomb force which prevents two electrons from coming too close to one another is almost the same as the exclusion principle, which prevents two electrons from occupying the same phase cell. As the Fermi-Dirac statistics requires the electrons to be indistinguishable, so does the Coulomb force. Eddington does not understand why his theory, which is so similar to the 'current theory' in which the exclusion principle and Fermi-Dirac statistics are accepted without questions, is 'still commonly alluded to as a 'bold speculation'.'<sup>87</sup> Eddington's arguments are extremely confusing, overlapping classical and quantum physics, bringing in descriptions of standing and progressive waves in order to invalidate the use of relativistic corrections when dealing with highly degenerate electrons. His aim is to discredit relativistic degeneracy completely and he is not afraid to attack the exclusion principle and Fermi-Dirac statistics in the process. Throughout his attack, he always describes the relativistic degeneracy formula as the 'current' formula about which he has great reservations. His attack is relentless and he seems almost perplexed that it is still being used when he has shown in his work that it is unsubstantiated and based on false premises built on the shaky foundations of quantum mechanics.

Regarding Eddington's treatment and analysis of the degeneracy formula, Kilmister writes,

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<sup>85</sup> See footnote 78.

<sup>86</sup> See chapter 2 footnote 22.



He was ready, in Bondi's opinion, 'to contort his physics' to retain Fowler's formula. He gave a new derivation of it in RTPE [*Relativity Theory of Protons and Electrons*] Chp. 13. It cannot be said to settle the matter; the argument extends over twenty-five pages but it is very obscure and to my mind not free of special pleading. Eddington's reputation was such that the Chandrasekhar limit was regarded with suspicion for something like twenty years. ... Eddington's enormous reputation in the early 1930s. It was at its peak. Once RTPE was published in 1936 the critics began to feel that something had gone wrong.<sup>88</sup>

It is not that Eddington is unsure about the fate of white dwarfs should there be a limiting mass. In fact he is well aware of what will result if a massive star exceeds Chandrasekhar's limit,

The star has to go on radiating and radiating and contracting and contracting until, I suppose, it gets down to a few km. radius, when gravity becomes strong enough to hold in the radiation, and the star can at last find peace.<sup>89</sup>

He has previously discussed the possibility of such extremely dense objects in *Internal Constitution of Stars* in 1926 explaining that Einstein's general relativity would predict that

a star of 250 million km. radius could not possibly have so high a density as the sun. Firstly, the force of gravitation would be so great that light would be unable to escape from it, the rays falling back to the star like a stone to the earth. Secondly, the red-shift of the spectral lines would be so great that the spectrum would be shifted out of existence. Thirdly the mass would produce so much curvature of the space-time metric that space would close up round the star, leaving us outside (i.e. nowhere).

Continuing humorously,

Lest this argument should be regarded by our more conservative readers as ultra-modern, we hasten to add that it is to be found in the writings of Laplace-

'A luminous star, of the same density as the earth, and whose diameter should be two hundred and fifty times larger than that of the sun,

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<sup>87</sup> Eddington (1936): 282-283.

<sup>88</sup> Kilmister (1994): 103,

<sup>89</sup> Eddington (1935a): 38.

would not, in consequence of its attraction, allow any of its rays to arrive at us; it is therefore possible that the largest luminous bodies in the universe may, through this cause, be invisible.<sup>90</sup>

This is clearly a description of a singularity or what we would now call a singularity or black hole. Chandrasekhar himself talks about Eddington's references to these implications which show that

Eddington realized that the existence of a limiting mass implies that black holes must occur in nature. But he did not accept that conclusion. He said that must be a *reductio ad absurdum*. Eddington's enormous physical insight clearly showed that black holes must occur once one accepted physics. If he had accepted that, he would have been 40 years ahead of anybody else. In a way it is too bad.<sup>91</sup>

But Eddington never once thinks that this is a plausible ending for a white dwarf star, although he is aware that they may exist. His preference was for a star to end its life as a stable white dwarf. The problem of energy and contraction which formed his paradox had continued to bother him until Fowler's application of the Fermi-Dirac statistics saved the stars from an uncertain ending. Chandrasekhar's limiting mass, however, revived the old problem, and Eddington's attempts show his desperation to get back to Fowler's neat solution. It would seem as though Eddington genuinely abhorred the idea of a black hole.

It is a perplexing point for Chandrasekhar who says in his Arthur Stanley Eddington Centenary Lecture in 1982 that he cannot understand why Eddington who was

one of the earliest and staunchest supporters of the general theory of relativity, should have found the conclusion that black holes may form during the natural course of the evolution of the star, so unacceptable.<sup>92</sup>

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<sup>90</sup> Eddington (1926/1988): 6. The term black hole was not *seriously* discussed until the 1960s when it was first coined by John Archibald Wheeler.

<sup>91</sup> Oral History Archive, Chandrasekhar (1977): 31; Chandrasekhar (1987): 135.

<sup>92</sup> Chandrasekhar (1987): 135.

It was, as we have seen, one of the predictions that could be drawn from general relativity, as Eddington had explained in *Internal Constitution of the Stars*. But why did Eddington find this so unacceptable? I will explore this question further in chapter five when I discuss the reasons behind Eddington's beliefs and action which prompted this controversy.

## CHAPTER FOUR: After the Controversy

Eddington's reaction to Chandrasekhar's results came as a great shock to Chandrasekhar. In this chapter I will discuss the aftermath of the controversy asking why Eddington had acted in this way, whether it was as unexpected as Chandrasekhar claimed, and probe the lack of support Chandrasekhar experienced after his encounter with Eddington. I will discuss in detail Chandrasekhar's correspondence with Dirac, McCrea and Rosenfeld whom Chandrasekhar had written to in a bid to garner support. This may enlighten us to the attitude of Chandrasekhar's peers regarding Eddington and the authority he wielded.

### 4.1 The Reluctant Astronomers

#### 4.1.1 Eddington's Unexpected Attack

Eddington's unexpected attack on Chandrasekhar's use of relativistic degeneracy astounded him. Why, thought Chandrasekhar, did not Eddington mention his dissent during the four months he had been meticulously completing his computations? There had been ample time and plenty of opportunity considering they were in contact at least three or four times a week.<sup>1</sup> In a close-knit community such as Trinity College where Fellows were expected to dine in hall routinely, Chandrasekhar and Eddington would have been thrust frequently into each other's company.<sup>2</sup> And even more puzzling was that Eddington had visited Chandrasekhar regularly to check his progress on his theory so there was definite communication between the two during this period, and specifically, regarding Chandrasekhar's theory.<sup>3</sup>

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<sup>1</sup> Wali (1991): 123.

<sup>2</sup> Letter of 13 June 1935 (Milne to Chandrasekhar), Box b428/D22, Milne Archive.

<sup>3</sup> Chandrasekhar (1977): 33, OHA, NBL.

After the controversy Chandrasekhar insisted, in public and in private, that he was shocked and hurt by Eddington's actions but, most of all, puzzled.<sup>4</sup> He offers no explanation for Eddington's sudden change in attitude and repeatedly asserts that he was genuinely surprised and that it was 'a totally unexpected occurrence, that nearly came to destroying my scientific confidence.'<sup>5</sup> Chandrasekhar had expected Eddington to be pleased with his results. After all, the existence of a limiting mass destroyed the basis of Milne's theory and would have confirmed Eddington's theory to be correct. And this was also something which Eddington anticipated. Thus Eddington's public attack at the RAS was completely unexpected. Chandrasekhar claims he had no idea of Eddington's opposition to the limiting mass. Because Eddington had shown a keen interest in his research and came regularly to check on his computational progress, Chandrasekhar had assumed that Eddington supported his conclusions and was on his side. Eddington was, after all, involved in a very public controversy with Milne at the time, and both astrophysicists were frantically trying to recruit support for their theories. And, more importantly, Eddington never said a word against Chandrasekhar's theory during this period and this may have been construed by Chandrasekhar as tacit acceptance on Eddington's part. Eddington's opposition therefore came as a complete shock to Chandrasekhar.

Although Wali agrees with Chandrasekhar's version of the story, in a large part because he is relying on Chandrasekhar's firsthand account, several others who have thought about this controversy in detail appear sceptical. The first is William Hunter McCrea who was present at the RAS meeting when the Chandrasekhar-Eddington

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<sup>4</sup> Chandrasekhar (1977): 34, OHA, NBL. Almost all of Chandrasekhar's friends and colleagues who were interviewed by the author stated that Chandrasekhar always claimed he was surprised by Eddington's reaction towards relativistic degeneracy. This is also stated in Wali (1991).

controversy first erupted. Like other professional and dedicated astronomers from around the country, McCrea religiously attended the monthly RAS meetings in London.<sup>6</sup> McCrea frequently met Chandrasekhar at the RAS meetings since 1932 when McCrea was still based at Edinburgh.<sup>7</sup> In the same year McCrea moved to Imperial College as Reader and Assistant Professor of Mathematics, and began to meet up with Chandrasekhar before the RAS meetings to have lunch together at South Kensington and then walk to Piccadilly for the meeting at Burlington House. About the RAS meetings, McCrea recalls:

The old meeting room was always just about full. I think that meant about 100 people. The astronomical community was more, in a way, integrated then, even than it is now. It's pretty well integrated still. But in those days everybody knew everybody else, and people like the Astronomer Royal felt, not only an inclination, but I think a duty to attend. So did Eddington and everybody like that. You could count on seeing everybody around. It was very nice. You'd see them all at tea-time.<sup>8</sup>

In his obituary of Chandrasekhar in the *Observatory*, McCrea voices his disagreement on the way in which Wali portrays the Chandrasekhar-Eddington controversy. This is due in part to McCrea's dissatisfaction of the negative portrayal of Eddington regarding the controversy in Wali's book.<sup>9</sup> Furthermore, in an interview in 1996, McCrea expressed strong doubts that Chandrasekhar was unaware of Eddington's views. If they had been in daily contact, McCrea believes that Chandrasekhar must have

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<sup>5</sup> Chandrasekhar, 'How I came periodically to change the area of my active interest after writing a book': 2, Box 1/Folder 1, Chandrasekhar Archive.

<sup>6</sup> McCrea (1993). McCrea complains that the standard of professional dedication towards the astronomical community is not as it used to be. Astronomers made more of an effort to participate in their professional gatherings so as to foster a community spirit. The hierarchical system within the community was also taken more seriously. Also in Wali (1991): 115. The status of an astronomer was revealed at the RAS meetings by how close to the podium the astronomer was seated. Thus people like Eddington and Fowler would sit at the front while Chandrasekhar and McCrea, who were only a few years into their research careers, would be placed in one of the back rows.

<sup>7</sup> Interview with McCrea (1996).

<sup>8</sup> McCrea (1978): 11, OHA, NBL.

<sup>9</sup> McCrea (1996): 123.

had some idea that Eddington was unhappy about Chandrasekhar's proof for a limiting mass. He may not have been aware of the *extent* of Eddington's opposition, and *that* may have come as a shock, but he must at least have been aware of some difference in opinion and direction of the research for which they were both aiming. For a man of Chandrasekhar's great intelligence, McCrea insists, it could not have escaped his notice. This sentiment is echoed in conversations with Takeshi Oka, a colleague and friend of Chandrasekhar's at the University of Chicago.<sup>10</sup> However, Wali accepts Chandrasekhar's insistence that he did not know about Eddington's views. It is possible that although Chandrasekhar and Eddington communicated over this matter it was purely on computational grounds rather than conceptual. However this argument is not entirely convincing as surely the whole point of doing the computations was to prove that the limiting mass existed. As both Chandrasekhar and Eddington were theoretical astrophysicists, the focus of their research would be on the theory rather than solely on the numerical computations.

In the obituaries published after Chandrasekhar's death, many recount the story of the controversy with Eddington, and what strikes the reader in each of the articles is the unanimous decision to cast Eddington as the villain of the piece.<sup>11</sup> This is partly due to the bully tactics Eddington employed in his public appearances, but I would put forward that the main reason behind such a negative characterisation of Eddington is that Chandrasekhar had ultimately been proved right, and this was formalised with his award of the Nobel Prize for Physics in 1983. Thus Eddington automatically becomes the astrophysicist who had made a major error in judgement and *unjustly* accused

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<sup>10</sup> Interview with McCrea (1996) and conversations with Oka (1998).

<sup>11</sup> Obituaries: See Abt (1995), Bethe (1995), *Daily Telegraph* (24 Aug. 1995), *Economist* (2 Sept. 1995), Lovell (1995), Lynden-Bell (1996), Mestel (1995), Rees (1995), Tayler (1995), *Times* (24 Aug. 1995).

Chandrasekhar of riotous speculation. This version of the controversy appears in print only after Chandrasekhar's award of the Nobel Prize.

Except in a few cases, the profiles of Chandrasekhar that were written prior to this award do not mention this episode in Chandrasekhar's life. But after the Nobel Prize and the public attention which comes to the recipient, the focus has mainly been on the controversy with Eddington. This is partly due to the nature of Chandrasekhar's award which placed strong emphasis on his earlier work on white dwarfs and the limiting mass. Thus in his acceptance speech Chandrasekhar finds it necessary to recount the events leading up to his discovery of the limiting mass, from Eddington's *Internal Constitution* to Fowler's astrophysical application of electron degeneracy.<sup>12</sup>

In a different form, the Japanese cartoonist Ryuji Tsugehara portrays Chandrasekhar's rise to fame and the Nobel Prize to the Japanese public using the controversy as a background. Employing the medium of manga or cartoon, and basing his story on interviews with Chandrasekhar, Tsugehara charts Chandrasekhar's struggle and his dismay when faced with Eddington's opposition. Eddington is shown to be a dedicated scientist struggling with his own theory of the universe who is unable to accept the concept of black holes. And although he is shown to be an unwavering opponent who manages to crush Chandrasekhar's attempts at proving his point, the cartoon concludes with the two scientists agreeing that although Eddington was wrong, he had battled against Chandrasekhar fairly, stressing that in Britain, what was paramount was the idea of *fair play*.<sup>13</sup> It is ironic that this is the one thing of which Lalitha Chandrasekhar denies Eddington was worthy. On Eddington's attack of

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<sup>12</sup> Chandrasekhar (1993).

<sup>13</sup> Tsugehara (1983). It is interesting to note that Chandrasekhar's story, especially after the Nobel Prize, is told in cartoon format to reach a wider audience, especially in Japan where comics, or manga, are in huge popular demand and are read by both adults and children.



Chandrasekhar at the RAS meeting, Lalitha wonders why Eddington did not say anything when he visited Chandrasekhar daily to check his progress, stating, 'That would have been sportsmanlike. But he kept it a secret and attacked without warning.'<sup>14</sup> Of course, the story used in the cartoon has been simplified even though it was based on an interview with Chandrasekhar himself. Chandrasekhar has also spoken publicly of the controversy in his Eddington Centenary Lecture which was delivered in 1982 and in several interviews when specifically asked about the controversy.<sup>15</sup> Chandrasekhar never discusses Eddington's behaviour and his feelings about the controversy in depth, but there is one letter to his brother, written after the controversial RAS meeting in 1935, in which Chandrasekhar vents his anger,

As for my work, it is going on but unfortunately, some controversies with Eddington and Milne during the past year has upset my enthusiasm a great deal. Milne is a sport and I like him. But Eddington is completely obscurantist. He is secure, though he does not understand quantum mechanics at all! Still my other physicist friends - Dirac, Bohr, Fowler and others - are solidly against Eddington's ideas and support me in my views. That makes things slightly easier, but nevertheless my controversy with Eddington has poisoned me and my peace quite considerably.<sup>16</sup>

Although there are several archival records which narrate the events during the controversy, apart from the letter quoted above, Chandrasekhar does not stray from his sterilised version of events, and in no way condemns Eddington for his actions, saying

We remained very good friends. But even now, when I think of him outside of the context of my controversy with him, I have the nicest feelings about him. But he was very, very obstinate. Very obstinate, right up to the end. ... But I don't think Eddington ever conceded. Oh no, he was convinced of his correctness right to the end.<sup>17</sup>

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<sup>14</sup> Chandrasekhar, L., 'Our Song', in Wald (1998): 274.

<sup>15</sup> Chandrasekhar (1987): 130-135; Chandrasekhar (1977), OHA, NBL.

<sup>16</sup> Chandrasekhar Archive, Letter to Balakrishnan (Chandrasekhar's brother), 20 December 1935, Box 7/folder 4.

<sup>17</sup> Chandrasekhar (1977): 29, 35, OHA, NBL.

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However, the idea of fair play within the context of academic and scientific debate and within the British culture is an inherently strong one. The concept of fair play is almost institutional, particularly in the British public school and Oxbridge psyche. Although scientific debates are fiery, those involved are always described as being on good terms and that it was only in the scientific arena that tempers frayed and friendships were put on hold. But once you left the arena, friendships were resumed and the debate was seen as scientific, rather than a personal, attack. We can also see this in Chandrasekhar's attitude towards the controversy. It would not be *fair* on Eddington if he described him in negative terms, especially since Eddington had passed away in 1944 and could no longer defend himself. Also it was not the correct way of conducting oneself. It is the same with Milne who maintains a correct friendship with Eddington although we have seen in his correspondence that he felt very bitter towards him. And in the interview with McCrea, we see that he also refuses to criticise Eddington. This is a testament to Eddington's great standing amongst his peers. He had accomplished an enormous amount in his career both in the academic and public arena: Eddington championed general relativity, he was one of the founders of theoretical astrophysics and he was a distinguished populariser of science.

But this also seems to be the way affairs were conducted within professional academic circles: one must never show one's true colours and must always maintain a professional attitude. Whatever method you choose to attack your opponent, it had to be within the strict rules of the academic community. Once you have left the scientific arena, it was inappropriate to continue your battle, and relationships must be resumed. As Chandrasekhar said in an interview, 'It did not affect our personal relations: that is

not the Cambridge Style!'<sup>18</sup> Of course, scientists being human, it was almost impossible to forget the humiliation and anger they felt when their research was attacked so publicly, and in front of their peers, as we have seen from private letters. And so their attacks become more veiled and hidden behind humour and wit. This was a method Eddington used to great effect when confronting his opponents, especially Jeans and Milne. Chandrasekhar himself was not spared any of Eddington's scathing wit, especially when Eddington proceeded to demolish Chandrasekhar's theory after what Chandrasekhar had thought would be a groundbreaking and triumphant talk.

As Chandrasekhar so succinctly wrote in a letter to Leon Rosenfeld in 1935,

Hardy asked me the question 'Is Milne deteriorating?'. 'It looks as though the question is at least pertinent' - that was my answer! - You see in Cambridge one learns to use bad language but politely!<sup>19</sup>

#### 4.1.2 Lack of Peer Support for Chandrasekhar

This unfortunate event was emphasised and prolonged by the lack of interest and support which Chandrasekhar encountered after the January meeting. Stunned after Eddington's talk, Chandrasekhar found that none of the astronomers questioned Eddington's arguments and all appeared convinced that Chandrasekhar's research was flawed. Amidst the whispers of condolences at the end of the meeting, Chandrasekhar turned for some support to Milne whom he had, shortly before the meeting, finally convinced of the validity of the limiting mass and hence the impossibility of compulsory degenerate cores. Regarding Eddington's verdict on relativistic degeneracy, Chandrasekhar recalls that Milne was 'all aglow' and had 'felt it in his bones that Eddington was right.'<sup>20</sup> Eddington's criticism had validated Milne's theory as it pointed

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<sup>18</sup> Chandrasekhar (1977): 29, OHA, NBL.

<sup>19</sup> Letter of 26 April 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

<sup>20</sup> Chandrasekhar (1977): 34; Chandrasekhar (1976a): 9; Wali (1991): 127.

out the inaccuracies of Chandrasekhar's reasoning. In the meeting Milne had addressed the audience after Chandrasekhar's talk to state that he had found results similar to Chandrasekhar's and which supported Chandrasekhar's theory. Yet after Eddington's talk, Milne quickly published a short letter in *Observatory* distancing himself from Chandrasekhar's now flawed theory in which he states,

In view of the fundamental character of the paper read by Sir Arthur Eddington ... perhaps I may be allowed to state that the basis of the calculation just completed ... is the equation of state  $p=k\rho^{5/3}$  for a degenerate gas. For the sake of simplicity, and to have a well-defined case fully worked out, we had restricted attention to composite configurations for which 'relativistic degeneracy', whether it exists or not, was ignored. Sir Arthur Eddington's investigations may now confer on our work a justification to which it is only accidentally entitled.<sup>21</sup>

Milne, however, does admit to the similarity of the aim and result of his work with that of Chandrasekhar. But the overall implication of this letter is to stress the differences which Eddington's talk addresses by cutting out the relevance of relativistic degeneracy completely. In fact, Milne even goes as far as stating that relativistic degeneracy may not even exist.

Of course, there is no reason why Milne should not have been pleased by this outcome. His own theory, after all, has been in the making since 1929, and although Eddington's outburst does not resolve his own controversy with Eddington, it is still in competition and not entirely dismissed out of hand. If Chandrasekhar's theory had been accepted, Eddington would have won his round against Milne outright.

There is no correspondence between Chandrasekhar and Milne regarding the controversy, and their friendship appears to remain unaffected. Chandrasekhar admits that he was very angry with Milne's response immediately after the RAS meeting, but

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<sup>21</sup> Milne (1935): 52; Chandrasekhar (1989): 137.

their correspondence remains frequent and undiminished touching on their respective work, although never quite discussing the controversy with Eddington.

The lack of support and inability to discuss this hugely important matter with his closest friends proved to be taxing for Chandrasekhar. From Milne's correspondence we can see that Chandrasekhar stops dining in hall in order to avoid unpleasant encounters with Eddington. Milne is naturally worried about this and advises Chandrasekhar not to be diminished by what had happened.<sup>22</sup> Milne himself knows firsthand what it must be like to be under attack from someone as powerful as Eddington and tries to persuade Chandrasekhar to regain his normal college life.

Chandrasekhar was initially unable to garner support from his peers who were present at the meeting as most of them were convinced by Eddington's arguments. But Chandrasekhar's extensive correspondence shows that he did not give up after the initial humiliation at the RAS and continued to write to his peers and kept on trying to justify the validity of his theory. In fact, many of his correspondents eventually came to side with Chandrasekhar. The only problem was that they were unwilling to do so openly and thereby enter into conflict with Eddington. McCrea in his obituary of Chandrasekhar denies that such is the case arguing that 'these days it is sometimes alleged that some astrophysicists at the time concealed their agreement with Chandra lest they should offend Eddington; this is nonsense historically since Eddington was even then regarding himself as the odd-man-out.'<sup>23</sup> Yet there seems no other reason why others who were experts on the subject of quantum mechanics should refuse to publicise their opposition if they agreed with Chandrasekhar and thought Eddington was mistaken in his arguments. The only other plausible reason is that this was a problem in astrophysics in

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<sup>22</sup> Letter of 13 June 1935 (Milne to Chandrasekhar), Box B428/D22, Milne Archive.

<sup>23</sup> McCrea (1996): 122.

which physicists did not want to get involved because it was a fringe subject. We recall that theoretical astrophysics was still a relatively new, and very mathematical, field in which only a small proportion of scientists were involved.

From the correspondence, the two scientists who provided extensive *private* written support were McCrea and Rosenfeld. Chandrasekhar also tried to garner support from Bohr, Dirac and Pauli who were the doyen of quantum mechanics, and although they did agree with Chandrasekhar's theory, stating that there was nothing incorrect in his use of quantum mechanics, they were unwilling to publicly certify this saying that astrophysics was not their forte as can be seen from this letter Chandrasekhar received from Leon Rosenfeld, then at Copenhagen, on January 29, 1935,

Now I am just sending you a few lines about the more pressing question of how to bring astrophysicists to reason. I vividly realise your troubles and feel very sorry for you. Bohr would [be] quite willing to help you, but he is very tired now and has to write two articles due for February 15th.; after this is completed, he intends to leave to some rest resort to recover from a very strained semester. He therefore feels it difficult to concentrate himself on a new subject just now; but he has a proposal to you, which I think would meet your wish in the best possible way. Would you agree [for] us to forward confidentially Eddington's manuscript to Pauli, together with a statement of the circumstances and asking for an 'authoritative' reply?<sup>24</sup>

Although both Bohr and Pauli were willing to help, and privately agreed with Chandrasekhar's version, they did not publish any material to support his theory.<sup>25</sup> As we can see from Rosenfeld's letter, there were several reasons why public support was not possible: lack of time, other academic commitments and the fact that it was not their field of expertise. Yet the crux of the argument between Chandrasekhar and Eddington was that Chandrasekhar had incorrectly fused quantum mechanics and special relativity, and that Eddington believed Chandrasekhar's understanding of quantum mechanics was

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<sup>24</sup> Letter of 29 January 1935 (Rosenfeld to Chandrasekhar), Box 27/Folder 6, Chandrasekhar Archive.

mistaken. As quantum mechanics was Bohr and Pauli's speciality, surely they would have been willing to give a definitive solution to this problem. Yet this did not happen.

Chandrasekhar did publish one paper with Christian Møller, an expert on quantum mechanics and relativity, in mid-1935 to defend his attack from Eddington.<sup>26</sup> Of the two letters from Eddington which survive pertaining to the controversy, one refers to this paper, with a request from Eddington who writes,

I am anxious to see a defence of the relativistic degeneracy formula published so that I can focus my attack. But it seems to me that there is too big a gap in your investigation to let it serve as a suitable basis of discussion. Would you look at the enclosed and see if you can do anything to meet the gaps I complain of? I am not refereeing the paper and this is entirely on a personal suggestion.<sup>27</sup>

The letter is accompanied by a note with the questions Eddington has for Chandrasekhar about his paper. Eddington's interest seems genuine and his request is courteous. The gist of the problem is what has driven Eddington from the beginning to refute Chandrasekhar's theory, namely that the premises Chandrasekhar uses do not make sense to Eddington. Eddington's understanding of Chandrasekhar's theory is that Chandrasekhar uses formulae for a degenerate electron gas in a finite volume without introducing standing waves. But to Eddington, the formulae would automatically mean the involvement of standing waves, and not progressive waves as Chandrasekhar suggests. But Eddington cannot understand the conditions which Chandrasekhar sets which would allow for progressive waves rather than standing waves as, in Eddington's opinion, would be normally assumed in this case. At this point Eddington identifies standing waves with the degenerate electrons.

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<sup>25</sup> Wali (1991): 131-2.

<sup>26</sup> Chandrasekhar (1935c) with C. Møller, 'Relativistic Degeneracy', *MNRAS*, **96**: 673-6..

<sup>27</sup> Letter of 12 June 1935 (Eddington to Chandrasekhar), Box 15/folder 1, Chandrasekhar Archive.

Only two other papers by physicists were published supporting relativistic degeneracy. The physicist Rudolph Peierls published a paper the following year on the derivation of the equation of state for a relativistically degenerate gas which was presented to the RAS by Chandrasekhar.<sup>28</sup> And Dirac himself published a paper in 1941 together with Peierls and Maurice Pryce, then at Cambridge, 'refuting Eddington's argument and proving that the boundary condition didn't matter and the shape didn't matter, and you could derive the states any way you liked.'<sup>29</sup> This may seem as though Chandrasekhar did not have any trouble looking for support from his peers in physics, but by the end of the 1930s, Chandrasekhar's theory was gaining popularity over Eddington's, and Dirac's contribution was entirely in keeping with the sway in opinion. That it did not come in 1935 when Chandrasekhar so desperately needed it probably contributed to Chandrasekhar's disappointment after Eddington's attack. However, Chandrasekhar insisted that he had a real feeling of support from physicists in Cambridge. It seems it was only the astronomers and astrophysicists who did not accept his theory.<sup>30</sup>

#### 4.1.3 Eddington's Authority

Can we explain this reluctance amongst Chandrasekhar's peers to publicly support him in his battle against Eddington? First of all there is a huge gulf in age, reputation and career between Chandrasekhar and Eddington. When the controversy began Chandrasekhar was twenty four years old and Eddington in his early fifties. So there is almost a thirty year difference between the two. Chandrasekhar has only just completed his PhD and was in the middle of his Trinity Fellowship while Eddington had

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<sup>28</sup> Peierls (1936).

<sup>29</sup> Peierls (1977): 12, OHA, NBL; Dirac, Peierls and Pryce (1941).



already established himself as one of the finest minds at Cambridge and the founding father of theoretical astrophysics, his monograph the *Internal Constitution of the Stars* already being a standard textbook for any serious astrophysicist. He was a participant in a number of high profile controversies with various scientists, notably Milne and Jeans, and had emerged unscathed. In other words, Eddington was a giant in the world of astronomy whereas Chandrasekhar was yet to make his name. This already places Eddington far ahead of Chandrasekhar in the game. Eddington has not been proven wrong before, and although he often followed his instincts sometimes without placing emphasis on the evidence, such as in the case of Einstein's prediction that massive objects will bend light due to the effects of general relativity, so far, his instincts have been proven correct. Ralph Kenat, who completed a dissertation on Eddington's role in the interpretation of the stellar interior, describes Eddington's method in the following way,

When Eddington turned to the interiors of the star he did so almost as if they were engineering problems; he wanted to understand how the star could be structured rather than what they were. He also was never driven by observational evidence as much as his own insights into stellar structure.<sup>31</sup>

McCrea wrote to Chandrasekhar a few days after the meeting,

Astrophysicists will not know what to believe. No one will understand Eddington, but some will accept his result on faith. The others will be in a state of confusion and will be disinclined to accept any result connected with the theory of degenerate matter, for all such results will be under suspicion of being upset by an 'Eddington effect'.<sup>32</sup>

It seems clear that many were inclined to believe Eddington on the strength of his reputation alone. And regarding this, Chandrasekhar says 'Men like Eddington had enormous personal prestige. But Eddington was the sole exception.' In fact, Eddington's

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<sup>30</sup> Chandrasekhar (1977): 38, OHA, NBL.

<sup>31</sup> Kenat (1987): 262.

reputation regarding his research was beyond that of almost all of the astronomers and astrophysicists in Britain and America at that time. To clarify Eddington's extraordinary status, Chandrasekhar contrasts him with Jeans saying, 'whatever respect people had for Jeans, in England, derived from his very successful ten-year tenure as the Secretary of the Royal Society.'<sup>33</sup> And Jeans was one of the great pillars of astronomy and astrophysicists, who sat towards the front row in RAS meetings, and who was frequently asked to referee papers for publication.

The second point is that Eddington was a great public speaker. Combining wit and analogy, he convincingly led his audience through his arguments, leaving them with a sense of completeness when he finally concluded his speeches, as McCrea describes to Chandrasekhar a few days later,

On Friday I thought Eddington sounded plausible, but now I cannot see where he can introduce any quantitative changes. For the sake of all your computations I can only hope he is mistaken! But I shall be anxious to hear what comes of it all.<sup>34</sup>

And in a letter to Wali, McCrea writes many years later,

When I listened to Eddington on this occasion I could not immediately weigh up all the implications of what he said, but my instinct seemed to tell me that he might be right...

What I am ashamed is not having tried to get to the bottom of the sort of argument Eddington produced. Had anyone other than Eddington produced such arguments, I suppose I should have done so. But they were superficially satisfying to me, and since they satisfied Eddington, I confess that I was content to let it go at that. In any case I was not working at stellar structure.<sup>35</sup>

We can safely assume that the majority of astronomers and physicists leaving the RAS that evening felt the same as McCrea. The last sentence stating that he was not working on stellar structure is true of almost everyone but a handful of astrophysicists

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<sup>32</sup> Letter of 10 February 1935 (McCrea to Chandrasekhar), Box 21/Folder 15, Chandrasekhar Archive.

<sup>33</sup> Chandrasekhar (1977): 39, OHA, NBL.

<sup>34</sup> Letter of 16 January 1935 (McCrea to Chandrasekhar), Box 21/Folder 15, Chandrasekhar Archive.

during this period. Milne had also made similar complaints regarding Eddington's capacity to convince his audience even with the weakest arguments.

Thirdly, theoretical astrophysics was a very new field in which Eddington was a giant. It was purely theoretical, very mathematical and steeped in controversy and confusion. As we have seen earlier in chapter one, the astrophysicists themselves grew confused in the controversies that sprang between Eddington, Jeans and Milne. The majority of the members of the RAS were observational astronomers. We have seen that the astrophysicists faced numerous problems when submitting their papers to the *MNRAS*. The observational astronomers were not seriously interested in the problems tackled by the astrophysicists. Many who were at the RAS meetings, including physicists and mathematicians, were there purely to witness the interesting debates and witty remarks which were thrown around. Thus when Chandrasekhar was trying to garner support, many felt unable to help him because it was not their field of expertise. As we saw in the case of Bohr and Pauli, astrophysics was also a fringe subject and Chandrasekhar believes that 'astrophysics was considered inferior by most physicists. In fact all physicists.'<sup>36</sup>

Finally, Eddington was also a successful populariser of science. To add to his collection of popular scientific books on astronomy, he began to write philosophical works about his theory of the physical universe. This also proved popular with the public as well as his scientific colleagues. In fact, Eddington was so popular that whenever he gave a public lecture, the *London Times* used to report it in full.<sup>37</sup>

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<sup>35</sup> Wali (1991): 134.

<sup>36</sup> Chandrasekhar (1977): 38, 40, OHA, NBL. The situation only began to change in the 1960 when research on black holes, gravitational collapse and relativity became popular.

<sup>37</sup> Chandrasekhar (1977): 50, OHA, NBL.

Eddington was by this time *the* authority on stellar astrophysics, an aristocrat in the realm of astronomy. As Chandrasekhar recalls,

He was a man who was very distinguished, in the sense that one felt when one talked to him that one was talking to someone really substantial. The British, particularly in earlier time, can be very nice and kind, but at the same time, an element in their behaviour makes it very clear that they're on a different level. There's no snobbery involved in it. It sort of comes naturally to them. Eddington was a man of that kind.<sup>38</sup>

And,

His position in astronomy was dominant, what Eddington said, was right. I don't think there was any doubt in anybody's mind that Eddington was always right.<sup>39</sup>

As Chandrasekhar recalls in an interview,

It is hard for people to realize what an incredibly dominating position Eddington had during his life. For example, Shapley told me this: in 1936, they had a tricentennial at Harvard, and, Shapley said, they send a circular around to American astronomers, to rank astronomers so they could give honorary degrees. And he said that Eddington was the first in every single list he received! And in one of them, Eddington [was at the top], 30 dots and then Jeans. I think that is most unfair, as far as Jeans was concerned, but the fact is that there was not a single astronomer in the thirties who would not with unanimity have said that Eddington is the greatest living astronomer. He had an absolutely dominating position.<sup>40</sup>

By the mid to late 1930s, Eddington's reputation as a philosopher somewhat palled as his colleagues found his books increasingly obscure and difficult to understand. Although his reputation as an astrophysicist was secure, his colleagues began to glimpse another side to his personality which they found difficult to interpret. Thus there may have been some physicists who thought that his later astrophysical work

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<sup>38</sup> Chandrasekhar (1977): 27, OHA, NBL.

<sup>39</sup> Chandrasekhar (1977): 28, OHA, NBL.

<sup>40</sup> Chandrasekhar (1977): 36, OHA, NBL.

was tinged with this obscurity, especially regarding his interpretation of quantum mechanics. As Cowling recalls about Eddington,

Oh, he had a very good intuition. It's only towards the end of the thirties, when he became interested in what he called "fundamental theory", where the number 137 turns up that he began to get his head so far in the clouds that you couldn't follow.<sup>41</sup>

#### 4.1.4 Racial Prejudice

Many have questioned the possibility of racism when trying to explain the white dwarf controversy. There is no evidence of racism in any of Chandrasekhar's papers, correspondence and interviews. In his Oral History Archive interviews in 1977 and 1987, he is asked very frankly by the interviewer about racism in both Britain and America. Although his accounts of racial prejudice in America is well known via his interviews and Wali's biography, there is no account of any such prejudice in Britain.<sup>42</sup> Chandrasekhar's answer to the question was simply, 'in England, I had no problems,'<sup>43</sup> and later elaborates that in Cambridge, 'we were treated, if anything, with more consideration that we thought we deserved.'<sup>44</sup>

Although there was no prejudice regarding his research and the controversy, and his friendships with Eddington and Milne, however, it was very rare for Indians to find a formal position at a British university. In fact Chandrasekhar is the first Indian to give lectures at Cambridge during his Fellowship in 1935 where he was paid £10 for a series of 20 lectures on 'Special Problems in Astrophysicists.'<sup>45</sup> Thus after his Fellowship at Trinity, Chandrasekhar was obliged to find a job elsewhere. He was advised by

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<sup>41</sup> Cowling (1978): 28, OHA, NBL.

<sup>42</sup> Chandrasekhar (1977): 54, OHA, NBL; Chandrasekhar (1987), OHA, NBL; Wali (1991); 204, 235-7.

<sup>43</sup> Chandrasekhar (1977): 53, OHA, NBL.

<sup>44</sup> Chandrasekhar (1977): 54, OHA, NBL.

<sup>45</sup> Letter of 15 February 1935 (Chandrasekhar to Father), Box 3/ Folder 9, Chandrasekhar Archive.

Eddington, who provided references and supported his applications, to look towards America. And when he received positions at both Chicago and Harvard, it was Eddington who advised him to go to Chicago.<sup>46</sup> Writing to his father after his decision to go to Chicago, Chandrasekhar says

On the whole I am convinced that America has been the first in recognizing me sufficiently to consider me worthy of an annual salary with a definitively senior position in a university - In India they are blissfully unaware of my existence, and in England though Eddington, Milne and Fowler have been awfully good to me, yet there is some prejudice in giving Indians a definite appointment tho' at Oxford, they have now appointed Radu Krishnan.<sup>47</sup>

It would seem that the Chandrasekhar-Eddington controversy did not have any racial implications. There appears to be no evidence to suggest this may have been the case and we can safely say that the controversy was on purely scientific grounds.

## **4.2 Chandrasekhar's correspondence with Dirac, McCrea and Rosenfeld**

After the RAS meeting in January 1935, Chandrasekhar wasted no time in contacting his peers who may provide support for his theory against Eddington's. Having encountered indifference and pity amongst those at the RAS meeting, he corresponded with several to see whether they could help him untangle the mess which Eddington had made. From his vast correspondence, there are only three people to whom he actually wrote about the controversy asking for their opinion and help. Only two letters from Dirac survive but there is quite a substantial number from McCrea and Rosenfeld who replied to Chandrasekhar's concerns in detail. From these letters, we can see that all three physicists agreed with Chandrasekhar that Eddington was wrong and

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<sup>46</sup> Chandrasekhar (1977): 58, OHA, NBL.

we can build a picture regarding how Eddington was perceived and also get an insight into how they tried to tackle this problem.

#### 4.2.1 Dirac

Dirac's biographer Helge S. Kragh maintains throughout his work, *Dirac: A Scientific Biography*, that Dirac thought very highly of Eddington and, like his contemporaries, almost seemed to hero-worship him. As a PhD student under Fowler, Dirac had to learn atomic theory and relativity. Atomic theory he studied using journals, Fowler's research and Sommerfeld's texts, and relativity, he learned from Eddington's *Mathematical Theory of Relativity*. He also attended Eddington's lectures and talks on special and general relativity and tensor analysis. When Eddington was scheduled to give an informal talk, there was always great excitement before and after the event, especially since it was generally followed by a heated debate. Dirac recalled that 'it was a really wonderful thing to meet the man who was the fountainhead of relativity so far as England was concerned.'<sup>48</sup> As we have seen, Eddington was instrumental in generating interest about general relativity in Britain. It was a new theory, revolutionary and startling in its originality, and for students, it was an exciting time, especially when they heard Eddington speaking with such flair and wit.

Dirac was also a member of the  $\sigma^2V$  (del squared V) Club for mathematical physicists where weekly discussions on new physical and mathematical theories took place. Members who were elected included Eddington, Milne, Fowler, Kapitza and Stoner.<sup>49</sup>

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<sup>47</sup> Letter of 23 April 1936 (Chandrasekhar to Father), Box 3/ Folder 11, Chandrasekhar Archive.

<sup>48</sup> Kragh (1990): 8-9.

<sup>49</sup> Kragh (1990): 10.

Chandrasekhar highly respected Dirac, who for a few months at the beginning of Chandrasekhar's PhD studentship was his acting supervisor, filling in for Fowler who was away on sabbatical. In the letters between Chandrasekhar and his father, Chandrasekhar frequently describes Dirac as 'perfect' explaining after a discussion with Dirac,

He was extremely encouraging and his high intellectual achievements have made him a perfect gentleman. This cannot be said of even Mr. Fowler or Professor Milne. Dr. Dirac is a class by himself.<sup>50</sup>

and

He is just wonderful! His philosophical insight into the general formalism of theoretical physics, his mathematical profundity to penetrate with ease any region of unexplained physical or mathematical thought and with all this what humility! He almost represents the PERFECT MAN - "almost" because of his such utter unconsciousness of his own depth (Dr. Dirac does not smoke or drink though I am not inclined to class these latter habits as vices for a Westerner.)<sup>51</sup>

And finally on a lighter note, Chandrasekhar had gone to see Dirac one morning and found him eating breakfast at around 10:30am. Seeing this, Chandrasekhar had written to his father, 'so I left him for a time, quietly happy that I had at last found in Dirac something human. ... But I had seen Dirac smearing marmalade on his toast and that is something to see after all.'<sup>52</sup> But he is also careful to explain to his father that Dirac had been studying until 5 o'clock in the morning. There are similar comments scattered throughout his vast correspondence with his father during Chandrasekhar's three years as a post-graduate student. We can see that Dirac, in Chandrasekhar's eyes, is someone to whom Chandrasekhar aspires, both as a person and a physicist.

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<sup>50</sup> Letter of 18 June 1931 (Chandrasekhar to Father), Box 3/ Folder 3, Chandrasekhar Archive.

<sup>51</sup> Letter of 22 January 1932 (Chandrasekhar to Father), Box 3/ Folder 5, Chandrasekhar Archive.

<sup>52</sup> Letter of 11 May 1932 (Chandrasekhar to Father), Box 3/ Folder 5, Chandrasekhar Archive.



Dirac is also a theoretical physicist who had, with his relativistic electron theory, successfully used relativity with quantum mechanics, and was therefore an ideal candidate to address his questions regarding Eddington's paper. Chandrasekhar probably saw himself as following in the footsteps of Dirac, albeit on a much smaller and more specialised case.

Only a handful of correspondence between Chandrasekhar and Dirac survive, of which only two are relevant to the white dwarf controversy. Both letters are replies to Chandrasekhar's questions regarding Eddington's talk against relativistic degeneracy at the January 1935 RAS meeting. Although copies of Chandrasekhar's letters to Dirac are absent from the Chandrasekhar Archive, Dirac's replies can be seen to soothe Chandrasekhar's queries regarding Eddington's opposition.

In his paper, Eddington had used a wave analogy to describe the different electronic motions in a perfect gas and ordinarily degenerate states. He had described electrons in a perfect gas as a progressive wave and those in an ordinarily degenerate gas (where pressure varies as  $\rho^{5/3}$  rather than  $\rho^{4/3}$  and no relativistic corrections are added) as standing waves. Therefore by introducing a relativistic factor into a perfect gas equation, one changed the motion of a progressive wave into a standing wave. This is unnecessary if what was being discussed was already a standing wave, or a degenerate gas. Therefore the introduction of the relativistic factor into the equation is made redundant. So in Eddington's opinion, there is really no need to bring in the relativistic factor *at all*.<sup>53</sup>

Dirac himself seems confused about Chandrasekhar's explanation of Eddington's arguments, but he assures Chandrasekhar in his letter of 12 February 1935 that Eddington's wave analogy is not a relevant argument,

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<sup>53</sup> Eddington (1935a): 39.

I cannot understand what you told me of Eddington's assertions. The Pauli principle, as it comes in in the general scheme of quantum mechanics, applies to any kind of states for the electrons, whether represented by stationary waves or not.

And regarding a quantum mechanical equation which Chandrasekhar utilises in his paper, he assures Chandrasekhar that,

I do not see any prospect of the equation ... getting modified by future developments of quantum mechanics, as it comes from such general ideas of counting waves in a box. ... I think you may continue to use [it] without worrying over it.

On the question of relativistic degeneracy, Dirac explains that if dealing with a non-saturated electron gas at very high temperatures, the production of positrons may have to be taken into account, but reassures Chandrasekhar by stating that

this would not affect your calculations for the case when the electron states of lower energy are all occupied, as then the negative-energy states would certainly all be occupied and there would be no positrons.<sup>54</sup>

Thus for a degenerate gas where all the phase cells are occupied, no complications with regard to positrons would arise. Therefore Chandrasekhar's equations can be kept simple.

Chandrasekhar had also sent proofs of Eddington's paper to Dirac for analysis.

He received a reply in the letter of 29 March 1935 in which Dirac writes,

I do not find Eddington's argument convincing and still favour the old theory. There is one definite objection that one may raise against Eddington's theory. If one alters the usual division of phase space into cells, this will be of importance not only when one has the Fermi statistics, but also when one has the Einstein-Bose statistics, which will upset Planck's law. The validity of Planck's law seems to me a strong argument in favour of the ordinary theory.<sup>55</sup>

Here Dirac uses the terms 'old' and 'ordinary' theory, when discussing Chandrasekhar's relativistically degenerate gas theory as opposed to Eddington's ordinarily degeneracy

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<sup>54</sup> Letter of 12 February 1935 (Dirac to Chandrasekhar), Box 14/ Folder 5, Chandrasekhar Archive.

gas theory. As he explains, his objection to Eddington's theory comes from a fundamental aspect of statistical mechanics, Planck's law.<sup>56</sup>

Only two letters from Dirac survive, and although he is not overwhelming in his support, it is clear in both letters that Dirac sides with Chandrasekhar regarding relativistic degeneracy.

#### 4.2.2 McCrea

Although Chandrasekhar had corresponded with McCrea over a number of years, there are only four letters of any significance regarding the controversy. McCrea was present at the RAS meeting on January 15<sup>th</sup> and they started corresponding immediately after the meeting. His letters are encouraging, admitting that Eddington's arguments are confusing and that on further detailed analysis, Chandrasekhar's paper does not seem to be incorrect. McCrea starts his side of the correspondence the following day with the paragraph,

I have looked up the Dirac paper you mention, and at any rate to my own satisfaction, checked the law as you indicate. I therefore find everything you say completely convincing, and agree that Eddington must have appealed to no new principle to derive his result. It is difficult from his few remarks on Friday to guess what this can be. ... On Friday I thought Eddington sounded plausible, but now I cannot see where he can introduce any quantitative changes. For the sake of all your computations I can only hope he is mistaken! But I shall be anxious to hear what comes of it all.<sup>57</sup>

This letter is followed by a more detailed analysis of Eddington's paper which Chandrasekhar had sent McCrea. The letter of 10 February 1935 begins with McCrea saying, 'in the first place, as far as I can see, he proves nothing!' He then lists

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<sup>55</sup> Letter of 29 March 1935 (Dirac to Chandrasekhar), Box 14/ Folder 5, Chandrasekhar Archive.

<sup>56</sup> Planck's law states that electromagnetic energy propagates in the form of photons or discrete quanta and is written as  $E = h\nu$  where  $E$  is energy,  $h$  is Planck's constant and  $\nu$  is the frequency.

<sup>57</sup> Letter of 16 January 1935 (McCrea to Chandrasekhar), Box 21/ folder 16, Chandrasekhar Archive.

Eddington's assumptions showing that Eddington's arguments are wholly based on *his* deductions which would naturally lead to *his* result. McCrea points out to Chandrasekhar that Eddington assumes the *classical* kinetic energy combined with the standard computation for degeneracy which will then give the inevitable result for ordinary degeneracy pressure instead of relativistic kinetic energy which includes relativistic corrections for speeds approaching the velocity of light. And regarding the exclusion principle McCrea writes that

[Eddington] says that the usual form of the exclusion principle is (a) while *he* assumes (b). Naturally he can wash out the relativity correction by putting a compensating factor into the exclusion principle used.

Here I think it is important to realise, as you certainly do (and as I am sure E. (himself) does even though he does not make it plain) that quantum theory is neither relativistic nor non-relativistic, but give rules for dealing with any system defined by a given Hamiltonian. But now it seems to me that the Pauli exclusion principle belongs rather to the general theory than to the part connected with the formulation of Hamiltonians, and so is unaffected by distinction between relativity and non-relativity mechanics. If we state it in the form that not more than one electron may occupy a given stationary state, then I think it is quite invariant ...

As regards the wave-equation, ... if he accepts that he must have thrown over Dirac's wave equation, and all existing relativistic quantum mechanics. And indeed unless I am very much mistaken, that is exactly what he has done. He complains of the unholy alliance of relativistic mechanics with non-relativistic quantum theory; he has squared the account by making the mechanics non-relativistic as well!

McCrea ends the letter by expressing his support for Chandrasekhar,

I will however continue to think about this subject, and if you think I can assist in any way in clearing it up I shall be glad to do so.\* For, after seeing E.'s paper I see more than ever the unsatisfactoriness of the position. Astrophysicists will not know what to believe. No one will understand E., but some will accept his result on faith. The others will be in a state of confusion and will be disinclined to accept any result connected with the theory of degenerate matter, for all such result will be under suspicion of being upset by an 'Eddington effect'.

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\*even to writing a (polite) discussion of E.'s paper sentence by sentence.<sup>58</sup>

From the letters in the archive, it becomes clear that Eddington is a figure whom his peers respect for his work, but whose work by this stage is always taken with a pinch of salt. As McCrea later writes in a letter of 20 February 1935 regarding a review he had to write of Eddington's *New Pathways in Science*, 'no matter how he may antagonize me on some points he never fails to entertain me too!'<sup>59</sup>

From his letters to Chandrasekhar, we can see that McCrea, probably like most of his peers, was persuaded by Eddington's arguments mainly because of Eddington's convincing delivery. It is only later, when questioned by Chandrasekhar that McCrea thinks back and realises that Eddington's arguments appear convincing only because they lead logically from his assumptions. Thus the root of the problem lies in the assumptions which Eddington makes regarding quantum mechanics and relativity. And this is where Chandrasekhar, Dirac and McCrea disagree with Eddington.

#### 4.2.3 Rosenfeld

Chandrasekhar's correspondence with Rosenfeld extended over a period of several years from 1932 to 1963. For the purpose of this thesis, only the letters which are relevant to the controversy and the theory of white dwarfs will be examined. These number over twenty and extend over a period from January to December 1935. These letters are very detailed and predominantly cover Chandrasekhar's research on white dwarfs and degeneracy. Chandrasekhar confided his troubles with Eddington to Rosenfeld, whom he had met in 1932 when he went to spend his summer in

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<sup>58</sup> Letter of 10 February 1935 (McCrea to Chandrasekhar), Box 21/ folder 15, Chandrasekhar Archive.

<sup>59</sup> Letter of 20 February 1935 (McCrea to Chandrasekhar), Box 21/ folder 15, Chandrasekhar Archive.

Copenhagen with Bohr and his team of quantum theorists.<sup>60</sup> He subsequently struck a strong friendship with Rosenfeld and this can be seen in their correspondence. The detailed way in which Rosenfeld tries to help Chandrasekhar, and the support he shows him is probably the greatest out of any of the physicists Chandrasekhar had approached. Of course we have to take into account that Chandrasekhar did not approach a vast number of his peers, only those whom he seemed to trust and who he thought understood his research. Even so, Chandrasekhar was a private man, and to have confided so deeply about his troubles to his select group of friends shows how deeply the controversy affected him. As Rosenfeld was based at Copenhagen with Bohr, Pauli and Dirac, who often visited the institute, he was in an ideal position to garner support for Chandrasekhar's theory. This is evident in his letters. Apart from the strictly academic nature of the letters pertaining to the controversy, we also find scattered throughout the letters jokes and light-hearted phrases which show their exasperation at the futility in their attempts to change Eddington's mind. Rosenfeld strongly supports Chandrasekhar and refers to his quantum theorist colleagues as doing the same. But as they are not astrophysicists, they can only do so by showing where Eddington has gone wrong in his application of quantum theory, but not in stellar theory itself.

After the January 1935 RAS meeting, Chandrasekhar immediately writes to Rosenfeld the following day asking for his help and for Bohr's opinion regarding this matter. He outlines the problem in detail and begins, 'I feel very guilty to pass on immediately to a matter which is of exceeding importance to me and on which I would like you to consult Bohr as well.' Chandrasekhar briefly outlines the main equations he applies in his theory:

Now for a completely degenerate electron gas one has the relations

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<sup>60</sup> Letter of 23 August 1932 (Chandrasekhar to Father), Box 3/ Folder 5, Chandrasekhar Archive.

$$n = 8\pi / h^3 \int_0^{p_0} p^2 dp$$

$$\xi = (8\pi / h^3) V \int_0^{p_0} E p^2 dp \quad \} \quad (I)$$

$$P = 8\pi / h^3 \int_0^{p_0} p^3 E / dp dp$$

where  $n$  = number of electrons per unit volume

$\xi$  = total energy

$P$  = pressure,  $V$  = volume

$E$  = kinetic energy of a free electron

$p_0$  = the 'threshold' momentum

$p$  = momentum of the particle.

Now on the relativistic mechanics

$$E = mc^2 [(1 + p^2 / mc^2)^{1/2} - 1]$$

From this and from the equation for  $P$  we get

$$P = (8\pi m^4 c^5) / (3h^3) \int_0^{\theta_0} \sinh^4 \theta_0 d\theta_0 ; n = (8\pi / 3h^3) p_0^3$$

where  $\sinh \theta = p / mc ; \theta_0 = \sinh^{-1} (p_0 / mc)$

From these we get the equation of state

$$P = (8\pi m^4 c^5) / (3h^3) [x(x^2 + 1)^{1/2} (2x^3 - 3) + 3 \sinh^{-1} x] \quad (1)$$

$$\rho = (8\pi m^3 c^3 \mu m_H) / (3h^3) x^3$$

where  $\mu$  = molecular weight

$H$  = mass of proton.

The question of importance is is (1) correct. Can one, put in I the relativistic expression for energy?

From (1) we deduce that

$$P \sim \rho^{4/3} \quad x \rightarrow \infty$$

$$P \sim \rho^{5/3} \quad x \rightarrow 0$$

The question whether the mode of derivation of (1) is in principle correct or not is of utmost importance to me.

The question Chandrasekhar wants to know is whether the application of a relativistic correction in the equation for the energy within the context of a degenerate gas is valid

or not. He had introduced a relativistic factor into the equation which would then alter the equation of state for a degenerate electron gas. He continues in the letter,

Because if one assumes (1) as correct then for a gas sphere in gravitational equilibrium with the  $(p, \rho)$  relation given by (1) then one can show that the structure is the simple differential equation

$$(1/\eta^2) d/d\eta (\eta^2 d\Phi/d\eta) = - (\Phi^2 - 1/y_o^2)^{3/2} \quad (2)$$

where  $\eta$  is the radius vector in a suitable scale and

$$\rho = \rho_c / [(1 - 1/y_o^2)^{3/2}] (\Phi^2 - 1/y_o^2)^{3/2}$$

$$\text{Central density} = \rho_c = B x_o^3 = B (y_o^2 - 1)^{3/2}$$

$$x^2 = y^2 + 1$$

I have spent two months in integrating (2) for different values of  $1/y_o^2$  and have worked out a complete theory of stellar structure based on (2).

Yesterday I gave an account of my work at the Royal Astronomical Society and after my paper Eddington sprang a surprise on everyone by saying the method of derivation of (1) was all wrong that "Pauli's principle refers to electrons as being stationary waves and that the use of the relativistic expression for energy in I is a misunderstanding." In fact according to him

$$P \sim \rho^{5/3} \quad (3)$$

the limiting  $\rho$  of (1) for  $x \rightarrow 0$ . ((3) is what one would get by using  $E = 1/2 mv^2 = p^2/2m$  in I.)

What Eddington objects to is Chandrasekhar's use of the relativistic expression for energy within the equation of state. Eddington is satisfied with the ordinary expression for energy which would give the ordinary equation for a degenerate gas where gas pressure is proportional to density to the power of 5/3. By using the relativistic expression, Chandrasekhar's relativistically degenerate gas pressure is proportional to density to the power of 4/3. Here we come to the core of the controversy. Eddington does not accept the relativistic correction which Chandrasekhar has made therefore



nullifying the concept of relativistic degeneracy and hence Chandrasekhar's theory.

Chandrasekhar continues,

If Eddington is right my last four months work all goes into the fire. But is Eddington right? Eqn (1) follows rig[o]rously from I and the relativistic expression for Energy. But can one use the relativistic expression for energy and combine it with Pauli principle as is done in I. This is of course an exceedingly important question of principle and I should very much like to know Bohr's opinion. Please consult him on the matter as soon as you possibly can and reply to me by air-mail. You can understand my anxiety if you know that I have been working out the consequences of (1) for the last four months working at an average rate of 12 hours a day and I do not want them to be all vain labour. I had hoped to feel joy of the work having been completed, but now Eddington says that (1) is wrong and I am terribly worried.

Kindly excuse me for giving you this trouble. But I am most anxious to have your and Bohr's opinion on the matter.<sup>61</sup>

We can see from this letter that Chandrasekhar's belief in his work is shaken.

Eddington has put doubt into his use of a relativistic correction within the quantum mechanical boundary of a stellar gas. In his work on stellar astrophysics, Eddington used only perfect gas models until he was persuaded by Fowler to include degenerate models for white dwarfs. Chandrasekhar and Milne were already dealing with degenerate gas models and to this Chandrasekhar had added relativistic corrections pushing the theory towards relativistically degenerate gas models. Eddington is clearly not satisfied, in fact is totally against, what he thinks is, Chandrasekhar's unnecessary use of relativistic corrections in a problem which can be solved solely by the use of quantum mechanics without the need for general relativity. So Chandrasekhar's next step is to consult the experts of quantum theory to see whether his addition of relativistic corrections for the degenerate gas is valid and who better than Rosenfeld and his colleague Bohr.

Like Chandrasekhar, Rosenfeld is surprised by Eddington's opposition to relativistic degeneracy replying in his letter to Chandrasekhar that 'nobody had ever

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<sup>61</sup> Letter of 12 January 1935 (Chandrasekhar to Rosenfeld), Box 27/ folder 6, Chandrasekhar Archive.

dreamt of questioning the equations' and that Eddington's remarks are 'utterly obscure.' He continues with 'you had better cheer up and not let you scare so much by high priests: for I suppose you know enough Marxist history to be unaware of the fundamental identity of high priests and mountebanks.'<sup>62</sup> From the tone of the letter, we can see that Rosenfeld cannot have been taking Eddington's views seriously.

This light-hearted vein when referring to Eddington continues throughout all the letters between Rosenfeld and Chandrasekhar with several references to the phrase 'high priests'. This phraseology is intriguing as it gives an insight to the hierarchy and roles played by various members within the scientific community. As we have seen in the case of Chandrasekhar in this controversy, and of Milne earlier, when it comes to the question of authority, strict hierarchical rules apply within the scientific community. In the case of astronomy we have the RAS, a generally conservative institution, which places great importance on academic status according to age and established reputation. Therefore Eddington, who already has established his reputation and is at the pinnacle of his career, is placed at the top of this hierarchy. At first instance, his authority and credibility is accepted unconditionally and Chandrasekhar is left to pick up the pieces after his encounter with Eddington. Even though his work outside astronomy may not be recognised or accepted by his peers, his authority within his own subject is absolute and unquestionable.

In the same letter, Rosenfeld writes that he submitted Chandrasekhar's request to Bohr, and upon examination, both found that Chandrasekhar's use of equations was correct. Regarding Eddington's argument about stationary and progressive waves, Rosenfeld adds that in the limit considered by Chandrasekhar both cases become equivalent resulting in the expression which Chandrasekhar has used for a completely

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<sup>62</sup> Letter of 14 January 1935 (Rosenfeld to Chandrasekhar), Box 27/ folder 6, Chandrasekhar Archive.

degenerate electron gas which is also relativistically invariant, ending the letter saying 'these quite obvious arguments would seem to settle the question without any doubt.'

Chandrasekhar's next letter to Rosenfeld on January 15<sup>th</sup> elaborates on Eddington's problem with Chandrasekhar's formulae. Upon talking to Eddington after the RAS meeting, Eddington told Chandrasekhar that the problem lies in the quantum mechanical aspect of the equations,

Let  $Z_{sd}Es$  denote the number of cells in phase space in the energy range  $Es$  and  $Es+dEs$ . Then on the unrelativistic theory

$$\begin{aligned} Z_{sd}Es &= (2V/h^3) 2\pi(2m)^{3/2} Es^{1/2} dEs \\ &= 2V(4\pi p^2 dp) / h^3 \end{aligned} \quad (A)$$

( $V$  = space volume).

Now is the formula

$$Z_{sd}Es = (2V/h^3) (4\pi p^2 dp) \quad (2)$$

general? If so then on the relativistic mechanics, since

$$E = mc^2 \{ (1 + p^2/m^2c^2)^{1/2} - 1 \}$$

$$\text{or} \quad p^2 = E(E + 2mc^2) / c^2$$

We have

$$Z_{sd}Es = (2V/h^3) 2\pi(2m)^{3/2} Es^{1/2} (1 + Es/2mc^2)^{1/2} (1 + Es/mc^2) dEs \quad (3)$$

Now Eddington thinks (3) is wrong. (From (3) the equations I gave in my last letter follow immediately.)

According to Eddington the relation

$$Z_{sd}Es = (2V/h^3) 2\pi(2m)^{3/2} Es^{1/2} dEs \quad (4)$$

is an identity. He reformulates Pauli principle in the way

"At most  $\{ 2(1 + Es/2mc^2)^{-1/2} (1 + Es/mc^2)^{-1} \}$  can occupy a phase cell of volume  $h^3$ ".

For  $Es \rightarrow 0$  the above reduces to Pauli principle. (The fast electrons exclude less!)

If his form of Pauli principle is right then from (3), (4) follows. Eddington says that relativistic invariance requires his formulation of Pauli principle.

Could you please give me yours and Bohr's view on this matter of principle.<sup>63</sup>

Eddington does not agree with Chandrasekhar's derivation of the number of cells in phase space for degenerate gas using relativistic mechanics. Instead, Eddington feels the need to reformulate Pauli's Exclusion Principle in such a way that what he ends up with is the non-relativistic equation. To this letter, Rosenfeld replies on the same day,

Bohr and I are absolutely unable to see any meaning in Eddington's statements as reported in your second letter. The question, however, seems quite simple and has certainly a unique solution. So, if "Eddington's principle" had any sense at all, it would be different from Pauli's. Could you perhaps induce Eddington to state his views in terms intelligible to humble mortals? What are the mysterious reasons of relativistic invariance which compel him to formulate a natural law in what seems to ordinary human beings a non-relativistic manner\*?

\*It seems to us as if Eddington's statement that several high speed electrons might be in one cell of the phase space would imply that to another observer several slow speed electrons, in contrast to Pauli's principle, would be in the same cell.<sup>64</sup>

It would seem as though even Bohr himself does not understand Eddington's arguments.

Upon further discussions with Eddington, Chandrasekhar writes to Rosenfeld on January 19<sup>th</sup>, 'I think I can now "explain" Eddington's difficulties with the usual treatments.' Eddington has difficulty accepting Chandrasekhar's description of electronic waves in a given volume. Chandrasekhar explains,

( I ) Consider a number of electrons in a given volume  $V$ . Then we have for the number of modes of vibration with wavelength  $\lambda$  the formula

<sup>63</sup> Letter of 15 January 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

<sup>64</sup> Letter of 15 January 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

$$2(4\pi\delta\lambda)/\lambda^4 \cdot V \quad (1)$$

(Eddington sees no difficulty in this). Now if in (1) we substitute De-Broglie's relation

$$\lambda = h/p = h/\sqrt{[m^2c^2(E/mc^2 + 1)^2 - 1]} \quad (2)$$

we get as we should expect

$$2 [(4\pi p^2 dp)/h^3]$$

$$E = \text{here represents the } \underline{\text{kinetic energy}} \quad (3)$$

Eddington says this procedure is wrong. "Relation (2) is for progressive De Broglie waves. But in (1) we have to use a relation for standing waves." I do not pretend to understand Eddington, but he objects to the passage from (1) to (3) through (2) - the "argument" being that they refer to two different things.

Chandrasekhar continues the letter with an example from Dirac who combines progressive waves to produce standing waves, and when the relativistic correction is added produces equation (3), same as above.<sup>65</sup> But Chandrasekhar writes, 'Eddington objects to this procedure. [Eddington says] "We cannot combine the wave functions to produce standing waves. They are incoherent. If two  $\psi$ -functions are written with a + symbol, we only mean that both are present. We cannot combine progressive plane waves to produce standing waves in the quantum theory."'

Eddington's main argument from his talk on 'Relativistic Degeneracy' which he gave after Chandrasekhar at the January RAS meeting centres around this idea that standing waves and progressive waves are two completely separate concepts which cannot be used in conjunction. Chandrasekhar (and Dirac) used it in their theories and Eddington objects to this. So we can see that Eddington is not opposing this solely because it is Chandrasekhar's theory, but more as a universal objection. He even goes as far as attempting to reformulate Pauli's exclusion principle to prevent such an

occurrence. In the case of the controversy, this leads to Eddington's rejection of the validity of the relativistic degeneracy equation and hence Chandrasekhar's theory.

Chandrasekhar continues in his letter to Rosenfeld,

I do not know if the above 'explanations' makes Eddington's point clearer - I do not understand him at all. But is it not possible to show that (3) is the expression one has to use by means of arguments which Eddington could understand or vice versa. Could one not show that if  $P \sim \rho^{5/3}$  [ordinarily degenerate gas] for all densities then there are contradictions like the one you point out in your last letter that "if several high speed electrons might be in a phase cell then it would result for [an] another observer that there are several slow speed electrons". I wonder if the argument could be made precise. He effectively modifies the Pauli principle for otherwise we would have  $P \sim \rho^{4/3}$  [relativistically degenerate gas] for high densities. Or could one show that  $P \sim \rho^{4/3}$  must hold for high densities for a degenerate gas and a theory which contradicts this must be self contradictory.

I am really sorry to trouble you, but Eddington is reading a paper at the colloquium next Friday and I want to have real missiles to throw at him! Some really simple way of demonstrating that any theory which shows that  $P \sim \rho^{5/3}$  is an identical proportionality for all densities must be necessarily self contradictory.<sup>65</sup>

Chandrasekhar is frustrated because he does not understand Eddington's arguments and even though he has had them explained to him, he does not see the reason in them and believes Eddington's reasoning to be self-contradictory. But it would seem that although he has been trying to convince Eddington, he does not seem to be succeeding in any way.

Rosenfeld writes back on January 23rd to encourage Chandrasekhar, although he is beginning to feel the futility of Chandrasekhar's fight against Eddington:

It seems to me that your new work is very important indeed, and I think everybody except Eddington will admit it rests on a perfectly sound basis.

As to the artillery fighting you are planning against Eddington, I could not imagine any missile more devastating than the one contained in

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<sup>65</sup> Dirac, *Proceedings of the Royal Society*, **112**: 661, §5.

<sup>66</sup> Letter of 19 January 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

my last letter. I feel a little dubious about the result of such a fight, since I do not expect Eddington, whatever the missiles, to collapse like a star with  $\beta_m = 1 - \epsilon$ ; it wouldn't be dignified enough for him to recant after he has gone so far as denying the existence of wave packets in quantum theory! Wouldn't it be a good policy to leave him alone, instead of losing one's time and temper in fruitless arguments? Nevertheless I wish you great fun next Friday, and I even regret not to be there to enjoy the show.<sup>67</sup>

But Chandrasekhar is not ready to bow to Eddington's demands on the validity of his theory and writes back on January 26,

I am afraid that I cannot leave Eddington alone! You can understand my disappointment. I have been spending months on my stellar structure work with the hope that for once there will be no controversy. Now that my work is completed, Eddington has started this 'howler' and of course Milne is happy. My work has shown that his (Milne's) ideas in many places are wrong, but my work depends on the relativistic degenerate formula and Milne can now go ahead. The result is there is going to be a long period of stress and confusion and if somebody like Bohr can authoritatively make a pronouncement in the matter it will be of the greatest value for the further progress in the subject. Already Fowler thinks that the relativistic formula (the one I gave in my letter to you and also quoted in my note in the *Observatory*) cannot be regarded as 'proved'.

So I have managed to get hold of Eddington's manuscript. He gave it to me and I am forwarding it to you for you and Bohr alone to read. I should be awfully glad if Bohr could be persuaded to interest himself in the matter - it is terribly important to settle the matter as quickly as possible, otherwise intense confusion would result in astrophysics - I am so sorry to trouble you over this but I hope you can understand.<sup>68</sup>

Because Eddington's main qualm is with the quantum theory that is used in Chandrasekhar's theory, Chandrasekhar is frantic to get an authoritative reply from Bohr, whom he considers the leading authority on quantum theory. With Bohr's backing, Chandrasekhar believes that Eddington may realise his mistake and take back his objections. As we can see from his letter, Chandrasekhar cannot find support in Cambridge, especially since the two people with the expertise and authority whose

<sup>67</sup> Letter of 23 January 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

<sup>68</sup> Letter of 26 January 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

backing could change the situation, Fowler and Milne, no longer support him. Unfortunately for Chandrasekhar, Rosenfeld writes back on January 29<sup>th</sup> with an apology from Bohr who is otherwise engaged and cannot spare his time. However, he continues,

[Bohr] has a proposal to you, which I think would meet your wish in the best possible way. Would you agree us to forward confidentially Eddington's manuscript to Pauli, together with a statement of the circumstances and asking for an 'authoritative' reply?<sup>69</sup>

Rosenfeld and Bohr are both sympathetic towards Chandrasekhar and his theory, and cannot understand nor accept Eddington's version of the quantum theory used. But, as we have discussed earlier, they were quantum theorists, not astrophysicists. With differing expertise, it is understandable that Bohr would be unwilling to make a definitive statement due to the constraints of specialisation, and also of time.

Chandrasekhar agrees to Rosenfeld's suggestion of getting Pauli's help. He points out in the following letter that the central problem is the validity of the expression

$$(4\pi p^2 dp)/h^3 \quad \text{with } E = \{(1+p^2/m^2c^2)^{1/2} - 1\}$$

in calculating the available amount of phase space. Chandrasekhar also informs Rosenfeld that Fowler is once again backing him and agrees that Eddington is wrong.<sup>70</sup> As we saw in Chandrasekhar's earlier letter, Eddington seemed to have convinced practically everyone, including Fowler who is an expert on statistical mechanics and one of the few people who are heavily involved with the Copenhagen crowd. Rosenfeld is surprised by Fowler's late backing, but his support of Chandrasekhar never wavers.

<sup>69</sup> Letter of 29 January 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

<sup>70</sup> Letter of 3 February 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.



Regarding Eddington's manuscript, Rosenfeld comments, 'having read [it] twice I have nothing to change to my previous statements: it is the wildest nonsense!'<sup>71</sup>

Chandrasekhar slowly begins to gather support from his peers, proclaiming to Rosenfeld in a letter of July 2 that 'one gets help from strange quarters!' when Jeans pushed through a publication of Chandrasekhar's work in the face of Eddington's opposition a few months later. This may, of course, be due to Chandrasekhar extending his research using Jeans' stellar model as the conceptual foundation for his paper. Chandrasekhar also recalled a meeting with Milne saying, 'he is very reasonable and agrees with my views' and 'he really hates Eddington!' And on meeting H.N. Russell, Chandrasekhar writes, 'he was frightfully enthusiastic. He finally whispered to me "Out there, we don't believe in E[ddington]"!! And Milne too as I said is very reasonable. He is a "sport". So finally I do feel a bit relieved.'

But Eddington is still engaged in the controversy, refusing to give way and was using quantum mechanical arguments to derive his version of Chandrasekhar's theory. But Chandrasekhar insists that by using the correct formula and Eddington's procedure, one eventually comes to Chandrasekhar's earlier formula for a relativistically degenerate gas, and so Eddington's premises are incorrect. On the publication of a joint note with Møller, Chandrasekhar writes, 'E[ddington] was very annoyed but there was no use!'<sup>72</sup>

Rosenfeld humorously replies, 'the story of Eddington's degeneracy (if I may use such an ambiguous expression) takes the shape of the Illia[d], with the various gods and heroes coming in.'<sup>73</sup> And in another letter, Rosenfeld discusses Chandrasekhar and Møller's paper agreeing with their conclusion and writes, 'it is of course equally easy to

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<sup>71</sup> Letter of 6 February 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

<sup>72</sup> Letter of 2 July 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive. Also letter of 12 June 1935 (Eddington to Chandrasekhar), Box 15/folder 1, Chandrasekhar Archive.

<sup>73</sup> Letter of 5 July 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

go on with this game, so to speak, of putting pressure on poor Eddington, by deriving the same result from Heisenberg's pair theory ...'<sup>74</sup> Rosenfeld seems to be mocking Eddington's knowledge of quantum mechanics. From the previous letters we can see that Eddington's arguments and use of quantum theory to sustain his opposition seem not to have made much sense to Rosenfeld. But even with Rosenfeld and the newly gained support of others, Chandrasekhar is still struggling to bring Eddington round to his point of view as we can see from his letter of 28 September 1935,

Eddington is completely adamant - the prefix "poor" you attach to his name is by no means proper! But I suppose it is really not worth while to 'go on with the game'. It is certainly more important to understand oneself the problems. It is no avail to attempt bending oak trees like Eddington.<sup>75</sup>

Chandrasekhar's correspondence with Rosenfeld regarding the controversy tapers off after the end of 1935, but the controversy itself continues with Eddington, although by now most of the main scientists who were consulted by Chandrasekhar such as Dirac, Fowler, Milne and even Jeans seem to be supporting him. In the last letter pertaining to the controversy sent by Chandrasekhar to Rosenfeld on December 7, Chandrasekhar writes,

Oh! By the way Eddington has now 'replied' [to] the note by Møller and me. He is completely crazy. He has said that the proof "conflict with wave-mechanics" - "uncertainty principle" too. I discussed with Dirac and he has asked me not to worry any more. I told Eddington that I could not agree with him, but also said that I had lost sufficient of my interest in the controversy to continue it any further with him! He was I think pretty badly shaken up! Milne is more reasonable. I spent a couple of days with him earlier in the Autumn - in October - but he was too tactful to commit himself. On the whole I am not sure if I am not more disappointed in Milne than with Eddington, for Milne could be persuaded to be reasonable while with Eddington it is quite impossible. So you see how the astrophysicists carry on! In any case but for the encouragement I have received from you, Strömberg and

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<sup>74</sup> Letter of 25 September 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

<sup>75</sup> Letter of 28 September 1935 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

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one or two others this muddle would have completely damped my spirits. I am glad to say that I am not quite extinguished yet.<sup>76</sup>

But Rosenfeld has not forgotten about the controversy when he writes a year later, 'I had a look through Eddington's new book. I find that he is not only stupid, and irritatingly conceited, but most unfair especially toward you. But I suppose it is not worthwhile to come back on that subject!'<sup>77</sup>

Rosenfeld's correspondence with Chandrasekhar was probably the greatest factor in encouraging and retaining Chandrasekhar's enthusiasm for what was initially thought by many to be a doomed research project. The words of encouragement and the jokes scattered throughout the letters probably managed to keep Chandrasekhar's feet firmly planted on the ground regarding Eddington's 'authority', and to show that the great astronomer was as fallible as the next scientist. When Chandrasekhar is leaving for Chicago, Rosenfeld sadly writes, 'thank you for your letter, which, however, as being written on the ship, has given me the melancholy feeling of initiating a wider separation from you. I tell you this without any sentimentality, just as an expression of the truth, namely that I have no other friend like you.'<sup>78</sup>

Rosenfeld, like the other quantum physicists, found Eddington's arguments incomprehensible and his grasp of quantum mechanics tenuous. His support for Chandrasekhar, as that of Bohr and Pauli, remains steadfast throughout although it does not extend into publicly demolishing Eddington's papers. Unlike the astronomers, Eddington's authority is not as solid amongst the quantum physicists as astronomers, many having already felt his obscurity to have tainted his research.

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<sup>76</sup> Letter of 7 December 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

<sup>77</sup> Letter of 11 November 1936 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive. Here Rosenfeld is discussing Eddington (1936), *Relativity Theory of Protons and Electrons*.

### 4.3 Final Encounters with Eddington

#### 4.3.1 IAU Conference in Paris 1935

Between 10 to 17 July 1935, the 5th General Assembly of the International Astronomical Union (IAU) was held in Paris. The President for the Commission of Stellar Constitution was Eddington and members included Emden, Fowler, Jeans, M. Pannekôek, Rosseland, Russell and Bengt Strömgren. Chandrasekhar also participated in the proceedings like many other astronomers who were not elected members. The main focus in 1935 was the future of subatomic processes such as the transmutation of metals and the liberation of energy and there were only three paragraphs referring to the commission headed by Eddington in the *Transactions of the IAU* published a year later. The last paragraph is pertinent to the controversy and was written with characteristic directness by Eddington,

The writer has recently contended that the “relativistic” degeneracy formula extensively used in stellar investigations is unsound and that the ‘ordinary’ formula is the correct deduction from relativity and quantum theory. A decision on this point profoundly affects the theory of super-dense stars.<sup>79</sup>

Following the reports, there was an hour-long discussion on the problems of stellar constitution with Eddington sitting as President and Russell as Secretary, but no details were recorded except a couple of sentences to this effect and that ‘the report as printed was unanimously adopted.’<sup>80</sup>

Chandrasekhar gives an account of this meeting in his interview for the Oral History Archive in 1977,

With Russell presiding, Eddington gave an hour’s talk criticising my work extensively and making it into a joke.

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<sup>78</sup> Letter of 30 December 1936 (Rosenfeld to Chandrasekhar), Box 27/folder 6, Chandrasekhar Archive.

<sup>79</sup> Stratton (1936): 238.

<sup>80</sup> Stratton (1936): 345.

I sent a note to Russell, telling that I would wish to reply. Russell sent back a note saying, 'I prefer that you don't.' And so I had no chance even to reply; and accept the pitiful glances of the audience.

...

I don't think that there was any doubt in anybody's mind in those days that Eddington was right, by virtue of Eddington's extraordinary dominance.<sup>81</sup>

Chandrasekhar had been preparing his defence against Eddington for the past few months, garnering support from the quantum physicists, even convincing Milne and Fowler of his theory's validity. Yet he was denied the chance to even answer Eddington's statement. Chandrasekhar was naturally upset by this, but he also recalled, as he had written earlier in a letter to Rosenfeld, that although Russell had refused him his chance to defend his theory, privately, when he met him earlier, Russell was frightfully enthusiastic about his work and had whispered to him, 'Out there we don't believe in Eddington.'<sup>82</sup> But it must have been discouraging to Chandrasekhar that this sentiment could not be announced in public and before Eddington. His distress over this meeting is evident decades after the event when Chandrasekhar received a letter from Horace Babcock on 23 August 1957 referring to a proposal for the IAU, Chandrasekhar penned a reply at the foot of the letter,

I have not [been to] any meeting of the IAU since 1935. I shall not go to Moscow. I am not interested one way or the other. I have resigned my membership.<sup>83</sup>

It is uncertain, however, whether Chandrasekhar actually sent this reply. But that he kept the letter with his comment gives us an insight into the anger he felt then and afterwards about the controversy and the unfairness of his treatment by his peers.

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<sup>81</sup> Chandrasekhar (1977), OHA, NBL. Also Wali (1991): 133-4.

<sup>82</sup> Letter of 2 July 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

### 4.3.2 International Conference on White Dwarfs in Paris 1939.

Chandrasekhar's first meeting with Eddington after his move to Chicago occurred at the 15th International Colloquium on Astrophysics which was held in Paris from 17 to 23 July 1939 and was also chaired by Russell. Chandrasekhar together with Eddington and Gerald Kuiper contributed to the Novae and White Dwarf section of the conference.<sup>84</sup>

Amos J. Shaler who was in charge of organising the conference writes to Chandrasekhar on 4 February 1939 regarding the allocation of subject matter. Eddington seems to be set on speaking about white dwarfs, which had initially been Chandrasekhar's allocated subject. But Eddington seems to have been unhappy about this decision,

I understand that Sir Arthur seems to have put up a fight concerning the change from white dwarfs to the more cosmological question. ...

Please do not think for a moment that you should abandon the white dwarfs, as a large number of the participant have voiced their desire to have you treat them rather than Sir Arthur. ...

If you have any trouble with Sir Arthur, do not fail to 'pass the buck' back to us, as it is not fair to have you shoulder any such burden.<sup>85</sup>

In fact, Eddington had already contacted Chandrasekhar regarding the conference. He had sent Chandrasekhar a letter on 22 January clarifying the subjects they would be discussing. Eddington has already decided that Chandrasekhar should discuss novae and Wolf-Rayet stars and himself white dwarfs as,

I have a certain amount to say, particularly as I think the subject has been obscured by the prevalence of the "relativistic degeneracy" heresy. If we stick to our original subjects I do not think we shall overlap, as if you refer to detail of white dwarf theory at all I expect we shall completely contradict one another.<sup>86</sup>

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<sup>83</sup> Letter of 23 August 1957 (Babcock to Chandrasekhar), Box 11/folder 5, Chandrasekhar Archive.

<sup>84</sup> Shaler (1941).

<sup>85</sup> Letter of 4 February 1939 (Shaler to Chandrasekhar), Box 24/folder 16, Chandrasekhar Archive.

<sup>86</sup> Letter of 22 January 1939 (Eddington to Chandrasekhar), Box 15/folder 1, Chandrasekhar Archive.

This conflict is not resolved immediately as Shaler writes to Chandrasekhar again on 11 February,

I was very much afraid that Sir Arthur Eddington would put up a fight. I am sincerely sorry. Professor Russell knows the circumstances, and, since, as you say, you will see him in Philadelphia next week, it would be a very good idea if you should talk it over with him. There may be a possibility of breaking up the two questions into two new ones which would satisfy both Sir Arthur and yourself, but I don't see just how that could be done. On the other hand, it would not be quite fair to ask Professor Russell to make an enemy out of either you or Sir Arthur. It seems to me, therefore that the best solution would be for either me or Mineur to make the decision and shoulder the weight of telling Sir Arthur that he is a cosmogonist and that he really should treat the question no. 13 [relations between novae, white dwarfs, planetary nebulae and Wolf-Rayet stars and their place in stellar evolution.].

I have asked Professor Russell to advise me of his opinions on the subject, but, with his usual generosity, I would not be surprised if he should decide to make a decision. As soon as I receive this information, I shall ask Dr. Mineur to do the dirty work right away. He started it anyway, by misinforming us in the first place, and he can damn well finish it. ...

I hope that this matter of Sir Arthur's will solve itself right away.<sup>87</sup>

In the end both Chandrasekhar and Eddington were given the opportunity to address the problem of white dwarfs, and Chandrasekhar somehow managed to prevent himself from being sidelined by Eddington regarding a subject which was very close to his heart.

Chandrasekhar's talk was before Eddington's and is short but straight to the point and is entitled 'The White Dwarfs and Their Importance for Theories of Stellar Evolution'. He immediately states that previously consequences of the Pauli principle and special relativity to the problem of white dwarfs were ignored and that the 'entire mass of the White Dwarfs must be degenerate'.<sup>88</sup> He discusses the evolutionary significance of such stars stating that for stars with masses greater than the white dwarf mass limit, 'since degeneracy cannot set in, in the interior of such stars, continued and

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<sup>87</sup> Letter 11 February 1939 (Shaler to Chandrasekhar), Box 24/folder 16, Chandrasekhar Archive.

unrestricted contraction is possible, in theory.’ However he also discusses the possibility of neutron stars and Wolf-Rayet stars where the star will decrease its mass below that of the limit by ejecting matter and supernovas. But he finished his discussion of this part of his talk by stating that his remarks on the evolutionary significance of the mass limit ‘are made with due reserve and no definiteness is claimed for them.’<sup>89</sup> Here Chandrasekhar is careful not to make any statements which he cannot support. In his research he has proved the existence of a limiting mass. But what happens to the star beyond that is still uncertain and he cautiously suggests the final outcome ‘in theory’. In the discussion that follows there is no reference to the validity of relativistic degeneracy and only questions clarifying the formulae and values used by Chandrasekhar. This strongly indicates that the astrophysicists have accepted Chandrasekhar's theory.

But Eddington's talk the following day on the ‘Theory of White Dwarf Stars’ addresses the very problem of relativistic degeneracy. In fact, as Chandrasekhar recalled to his biographer Wali, Eddington began his talk by saying, ‘our beliefs on Saturday must be different from our beliefs on Friday’ and proceeded to give his version of the theory.<sup>90</sup> He does not question the formula and existence of complete degeneracy in white dwarfs. But as he discusses Stoner and Anderson’s relativistic degeneracy formula which they discovered in 1930, he states with regard to the addition of relativistic effect that ‘the modification is, however, fallacious; and it now appears that a rigorous treatment leads to the original equation [i.e. non-relativistic degeneracy].’<sup>91</sup> He continues,

Clearly astronomical progress is altogether dependent on knowing the correct equation of state; and since this is not yet looked upon as non-

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<sup>88</sup> Shaler (1941): 41-2.

<sup>89</sup> Shaler (1941): 47.

<sup>90</sup> Wali (1991): 137.

<sup>91</sup> Shaler (1941): 52.



controversial, it seems necessary to devote some part of this Report to examining the physical theory of the pressure-density formula. It is impossible within reasonable limits of space to develop fully the considerations, going down to the roots of quantum theory, which seem to lead definitely to the exact formula  $P = k\rho^{5/3}$ . The discussion can, however, be carried far enough to show the extravagant defects of commonly accepted forms of treatment (especially those which yield the Stoner-Anderson formula which rule them out of consideration as a basis for astronomical theory. ...) The crucial part of the problem is to obtain a mathematical formulation which correctly embodies the physical conditions; it is in this respect that many of the published investigations fail.<sup>92</sup>

And regarding Chandrasekhar's work, Eddington flatly states, 'Chandrasekhar's investigation must be rejected because his mathematical formulation does not in any way correspond to the physical problem.'<sup>93</sup> Even though Chandrasekhar's mathematical derivation cannot be faulted, the physical assumptions from which he starts are already not complementary to the problem. As we have seen, Eddington believes Chandrasekhar's use of special relativity and the Pauli Exclusion Principle is unnecessary and wrong. His fundamental mistake, in Eddington's view, in using progressive waves in place of standing waves invalidates the theory right from the start. Eddington then quickly moves on to his discussion of white dwarfs regarding energy liberation and evolution.

In the discussion following Eddington's talk, Chandrasekhar brings up the problem of relativistic degeneracy and the following is recorded in the summary of the discussion,

Sir Arthur Eddington replies that in stars of mass greater than the critical masses mentioned by Dr. Chandrasekhar there is no limit to the contraction, so that if the star is symmetrical and not in rotation, it would contract to a diameter of a few kilometers, until, according to the theory of relativity, gravitation becomes too great for the radiation to escape. This is not a fatal difficulty, but it is nevertheless surprising; and, being somewhat shocked by the conclusion, Sir Arthur was led to

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<sup>92</sup> Shaler (1941): 52-3.

<sup>93</sup> Shaler (1941): 55.

reexamine the physical theory and so finally to reject it. Whatever Fowler's view of the paradoxes may have been, he eliminated the difficulty by showing how the contraction could be stopped and how the star could become cool again.<sup>94</sup>

What Eddington is describing here is what we now know as a black hole. Eddington is well aware that something of this description will result if Chandrasekhar's theory is accepted, although exactly what it may be is unknown. And he immediately rejects the conclusion. Regarding the phrase 'whatever Fowler's views of the paradox may have been', Eddington is aware that Fowler no longer agrees with him. Chandrasekhar's theory reintroduces the paradox which Eddington and Fowler had removed earlier with the introduction of electron degeneracy to stabilise white dwarfs. With relativistic degeneracy and the mass limit white dwarfs are no longer stable and will continue to contract. As Chandrasekhar recalls, Eddington 'went even to the extent of saying that Fowler himself did not understand his own formulation of the paradox. "Fowler is a mathematician - he does not understand physics," Eddington said.'<sup>95</sup> The discussion continues with Eddington stating,

As was seen yesterday, Stoner and Anderson reintroduced the type of star that cannot cool down. No experimental test of these purely theoretical questions is possible; no one is required to choose between the two theories; but, having eliminated one of them, it would be useful to try experimental tests, to show whether modifications in the survivor are necessary, though probably the experimental difficulties are too great to lead to a conclusion.<sup>96</sup>

On being questioned further, Eddington admits that his main difficulty in accepting the Stoner-Anderson formula is 'of having to suppose that a star of ordinary mass would contract almost to a geometrical point.'<sup>97</sup> Further in the discussion it is recorded:

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<sup>94</sup> Shaler (1941): 65.

<sup>95</sup> Wali (1991): 137.

<sup>96</sup> Shaler (1941): 66.

<sup>97</sup> Shaler (1941): 68.

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Observer Kuiper asks Sir Arthur Eddington if there are any observational tests that would permit a choice between the two rival theories. Sir Arthur insists that the Stoner-Anderson formula does not exist, observation can decide between rival hypothesis but not between rival conclusions which profess to represent the same hypothesis.<sup>98</sup>

Eddington has already rejected Chandrasekhar's theory and with it the Stoner-Anderson formula. To him, there is only one theory of white dwarf stars, and that is the one he, with Fowler, had formulated in 1924. In fact, we can go as far as saying that there is no possibility at all for Eddington that Chandrasekhar's theory could be correct and he makes it very plain in his talk and in the discussion that follows, although several of his peers are enquiring about the 'non-existent' rival theory.

Chandrasekhar recalls, "at this point, I got very angry. I got up and said, "Well, Eddington, how can you say that there are no two theories? Because we were in Cambridge just the other day, in a discussion with Dirac and Peierls and Maurice H. Price, and all three did not agree with your work on degeneracy. And to the extent that these distinguished physicists think that my formula is right, an observational astronomer must conclude that there are two theories."<sup>99</sup>

Although Eddington never concedes, Chandrasekhar does not give up and the last line recorded in the discussion states: 'Dr. Chandrasekhar defends the Stoner-Anderson formula and suggests that for the observer the two theories must be considered rivals.'<sup>100</sup> This is a big change from the previous IAU conference on white dwarfs in 1935 when Chandrasekhar was not even allowed to give his defence. In this conference, however, Chandrasekhar *is* allowed to make his case, question Eddington several times (and this is recorded in the discussion) and have the last word.

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<sup>98</sup> Shaler (1941): 69.

<sup>99</sup> Chandrasekhar (1977): 35, OHA, NBL; Wali (1991): 137.

<sup>100</sup> Shaler (1941): 69.

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So what has changed? Eddington is still present, Russell is again chairing the discussion and the astronomers who are present also participated in the previous conference. From the letters Shaler had sent to Chandrasekhar before the conference, it is clear that he is sympathetic to Chandrasekhar's research and, as the editor of the report, he makes this very clear. He also states in his letter that several of Chandrasekhar's peers would rather hear Chandrasekhar speak on white dwarfs than Eddington. And from the previous conference in 1935, we know that Russell was privately supporting Chandrasekhar's theory, and this time, he did not let his respect of Eddington's authority overrule his support. Eddington is also seen more as a cosmogonist now rather than a straight astrophysicist, his main work in the 1930s concentrating more on cosmology and fundamental theory.

In fact, by this stage, the majority of astronomers whose work were involved in the controversy had swung round to Chandrasekhar's side. As we have seen earlier in Chandrasekhar's letters to Rosenfeld, Milne and Russell both praised Chandrasekhar, Bohr and Pauli agreed with him and Dirac was also offering support. Fowler also accepted Chandrasekhar's theory and had told Chandrasekhar, 'Don't worry about Eddington.'<sup>101</sup>

This is Chandrasekhar's last meeting with Eddington before WWII and Eddington's death in 1944. Wali describes their last meeting as 'poignant' and Chandrasekhar recalls that he was sitting at lunch with all the great French scientists, alone and angry after the discussion. 'After lunch I was standing entirely by myself waiting to leave in the next hour to take a train to Cherbourg ... Eddington suddenly appeared next to me. He said "I am sorry if I hurt you this morning. I hope you are not angry with what I said." I said, "You haven't changed your mind, have you?" "No," he

said. “What are you sorry about then?” I said and turned away. Eddington sort of stood there for a few moments and walked away.’ Chandrasekhar tells Wali that he has always regretted his last words to Eddington, ‘I was rude, was unforgiving when he came ... essentially to apologise.’<sup>102</sup> Chandrasekhar never saw Eddington again, but they exchanged a friendly correspondence until the end of 1943, the year before Eddington's death.

In 1953 Chandrasekhar received a letter from C.J.A. Trimble, a close friend of Eddington's, who was collecting anecdotes for a chapter on Eddington. By this time, Eddington's one-time student, Alice Vibert Douglas was already embarking on her biography of Eddington and has asked Trimble for his help. This is probably for his chapter in her monograph although Trimble was unable to complete it due to poor health. He asks Chandrasekhar for a contribution, stating in the letter of 26 May,

Dr. Evans of the Cape, S. Africa (who was I think at Pretoria before) mentions a story about you. He said “On the publication of Chandrasekhar's book on Stellar structure, he (i.e. Eddington) is reported to have murmured – ‘How nice to have all the wrong things in one place?’ ... Will you allow me to include in the chapter this whimsicality?”<sup>103</sup>

In reply, Chandrasekhar quotes a review of his book *Principles of Stellar Dynamics* which Eddington had written in *Nature*, in 1943:

As a subject progresses the attractive simplicity of the early researches gives place to laborious elaborations. In the last three years, Dr. Chandrasekhar has been very active in the mathematical development of stellar dynamics. The trend of his work may be judged from the fact that one contribution alone contains more than 1,800 numbered formulae. There is no denying that this heavy method of attack can be justified; but it leaves us with the depressing feeling that the subject

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<sup>101</sup> Chandrasekhar (1977): 38, OHA, NBL.

<sup>102</sup> Chandrasekhar (1977): 35, OHA, NBL; Wali (1991): 138.

<sup>103</sup> Letter of 25 May 1953 (Trimble to Chandrasekhar), Box 15/folder 6, Chandrasekhar Archive. There is speculation that Trimble was actually Eddington's partner. Although there is no direct evidence that Eddington was homosexual, several researchers including J. Eisberg (who speculates in her doctoral thesis) and A. Warwick (in conversation) have expressed their opinion that they believe this to be the case.

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which began thirty years ago as a joyous adventure has reached a stage of uninspiring ugliness.

To which Chandrasekhar remarks, 'This is typically Eddington.'<sup>104</sup>

## 4.4 Social interactions and group dynamics

### 4.4.1 Group Dynamics within the Chandrasekhar - Eddington Controversy

As we have seen in the preceding chapters, there was a great deal of subtext within the group dynamic between the astrophysicists. First there was the Eddington - Jeans - Milne triangle where Milne was the initial outsider, and therefore, the target for support. Both Eddington and Jeans were the heavyweights battling out their theories at the RAS in the early 1920s. In 1929 Milne joined the fight effectively pushing Jeans aside and facing Eddington with his own stellar theory. By the time Chandrasekhar arrives on the scene, the Eddington - Milne controversy was reaching a bitter impasse, and Chandrasekhar was the next target for support. Although working closely with Milne, Chandrasekhar's theory in fact supported Eddington's stellar model.

Throughout the controversy of the limiting mass run strands of earlier controversies between Eddington, Jeans and Milne. Since 1924 Eddington and Jeans had been arguing over the constitution of the stars, whether they were polytropic (completely gaseous throughout and obeyed the ideal gas law) or liquid. Milne entered the arena with his Bakerian lecture in 1929, questioning Eddington's method of attacking the problem of stellar constitution and evolution, and introduced another candidate for stellar structure: stars with degenerate cores but ideal gas envelopes. Milne was already aware of the situation he was entering and first took Jeans' side. But he soon

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<sup>104</sup> Letter of 24 June 1953 (Chandrasekhar to Trimble), Box 15/folder 6, Chandrasekhar Archive. The review can be found in *Nature*, 151: 91 (1943).

criticised Jeans' theory and was rebuked by de Sitter. Jeans retorted to Milne's entry by stating that there were only two possible stellar structures, polytropic or liquid, further aggravating the situation.<sup>105</sup>

This was the background into which Chandrasekhar entered with his exact theory of white dwarf stars, and he promptly found himself in the centre of the earlier controversies, trying carefully not to take sides. But it is already too late. By getting involved in the problems of stellar structure, Chandrasekhar's position was already being evaluated. Chandrasekhar recalls,

At the time I went to see Eddington, I had already published two papers in the *Monthly Notices*. So Eddington from the first regarded me as an ally of Milne's.<sup>106</sup>

Eddington has already placed Chandrasekhar in Milne's camp. And likewise, Milne who was working intensively with Chandrasekhar felt the same, pushing Chandrasekhar to criticise Eddington's work and to support him when Chandrasekhar presented his theories. Milne's backing off after the January 1935 RAS meeting must have seemed a great betrayal indeed.

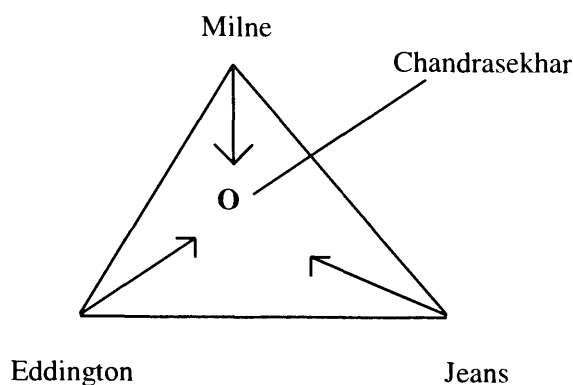
And in a letter to his father on 9 February 1935, Chandrasekhar writes,

My last paper on stellar structure involved me desperately into the rival jealousies of Eddington-Milne-Jeans. I am taking care to be scrupulously polite to all of them. Fortunately Fowler and Bohr are on my side. I cannot properly go into these matters in a letter. It is the continuation of the history of my differences in attitude in results with Milne which has been brewing for the last three years. The explosion hasn't yet occurred. The whole thing would have been smoothed over had it not been for an awful howler Eddington has started. He (ASE) thinks that Pauli's principle is wrong! I do not know what he is up to. But the scientific politics in stellar structure in the triangular contest Eddington, Jeans, Milne strains me as I am in the middle and refuse to take anybody's side and E, M and J [initial's are Chandrasekhar's own] are all quarrelling with my work!!\*

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<sup>105</sup> Referred in detail in chapter one.

<sup>106</sup> Chandrasekhar Archive Box 2/Folder 11.



But Fowler supports me from the top.<sup>107</sup>

Chandrasekhar himself uses the phrase ‘rival jealousies’ to describe what fuels the astrophysicists to perfect their theories. By the time Chandrasekhar is drawn into the bitter controversy, it had been slowly fermenting for over a decade. In some ways, the astrophysicists saw Chandrasekhar as a pawn to be used to validate their own theories and Chandrasekhar himself is aware of the ‘scientific politics’ which underlies the controversy. Chandrasekhar’s use of the phrase ‘scientific politics’, in his letter to his father, is extremely significant. It highlights his enlightened attitude towards the situation in which he finds himself and shows that he is aware and ready to participate in the controversy. It also signifies the acceptance by his peers of his professional scientific status: by joining in the gentlemanly debate within the scientific arena, he has been accepted and has become a legitimate astrophysicist. Here are all the great names in astrophysics, whose work he had studied as a student, calling upon him to validate their theories. They are checking to see his progress and have even implied that on his results may hinge the validity of their own theories. And, in addition, he has also corresponded with Bohr, Dirac and Fowler, the stalwart champions of quantum mechanics, to garner their support. Although Chandrasekhar seems to show that he is above the scientific

<sup>107</sup> Letter of 9 February 1935 to father. Box 3/ Folder 9, Chandrasekhar Archive, University of Chicago.



jealousies and politics involved, we can clearly see that he is happy to be, and even relishes being, included in the debate. For a young scientist recently having completed his PhD and a newcomer to the scientific establishment, it must have seemed a high honour indeed.

Chandrasekhar's naiveté is also undergoing a transformation. When he began his scientific career as a Fellow of Trinity, Chandrasekhar was still only 22 and he was thrust into his controversy with Eddington only a few years later. He was still in awe of the 'great' scientists, their weaknesses and human characteristics still invisible to him. But it would seem that Chandrasekhar has learnt that a scientist's path is not one noble trajectory upheld just by his research, but that he must deal with the internal politics which are inherent in academia and the scientific community. The next part of his letter shows how he has become more skillful in ensuring that his research and papers are accepted,

By the way, you will have seen that in my second "Stellar Configurations" paper, I have been nice to Jeans. It was all politico! The RAS I knew would have been reluctant to publish it (my paper), but I knew also that to save their faces they would send it to Jeans hoping to get a bad report! - then they can turn down the paper with no qualms! I was aware of this, and so I referred to Jeans very nicely. The trick worked! Jeans was emphatic about the publication! - It was all really sickening - these underhand methods, but what can one do?<sup>108</sup>

#### 4.4.2 Aftermath of the Controversy

From January 1935 until the death of Eddington in 1944, the controversy raged on with Eddington becoming increasingly more scathing in his remarks. Chandrasekhar himself decided to move on and switched research fields in 1939 after publishing his

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<sup>108</sup> Letter of 28 September 1935 (Chandrasekhar to Rosenfeld), Box 27/folder 6, Chandrasekhar Archive.

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work on white dwarfs in monograph form as *An Introduction to the Study of Stellar Structure*.

Towards the end of 1936 Chandrasekhar moved to Yerkes Observatory to join the Department of Astronomy at the University of Chicago. Until then he had tirelessly argued with Eddington and had corresponded extensively with colleagues such as Dirac, McCrea and Rosenfeld in order to gather support and to construct credible arguments to convince Eddington of the existence of relativistic degeneracy. Although Bohr and Pauli agreed with Chandrasekhar's analysis and could make no sense of Eddington's, they were reluctant to publicly pronounce on the subject. No one was willing to challenge Eddington's authority.

After the January 1935 RAS meeting, Chandrasekhar did not pursue the controversy with Eddington apart from on two occasions. The first was at the International Astronomical Union (IAU) meeting at Paris in 1935 where to Chandrasekhar's dismay and anger, he was not allowed to reply to Eddington's criticisms.<sup>109</sup> And the second occasion was at the International Conference on Novae and White Dwarfs, also in Paris, in 1939. Eddington was present and spoke vehemently against relativistic degeneracy. There ensued a fiery debate between Chandrasekhar and Eddington with neither giving way.<sup>110</sup> Eddington continued to speak against the theory and wrote several articles to this effect until his death in 1944.

McCrea, Lalitha Chandrasekhar, and Chandrasekhar himself have all denied that Chandrasekhar harboured any bitterness or ill-feeling towards Eddington. Looking through his articles and talks he had given over the years, Chandrasekhar's account of the controversy and of Eddington is homogenised and never falters. Yet, one cannot

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<sup>109</sup> Chandrasekhar (1977), OHA, NBL. Also Wali (1991): 133-4.

<sup>110</sup> Shaler (1941): 64-69.

help wondering whether in actuality this was so. McCrea's student, Derek McNally, who spent a year working with Chandrasekhar at Chicago in the 1960s, is not so sure. He recalls that when the conversation eventually got to Eddington and the controversy, Chandrasekhar's reaction was different from that given on paper. He was still very bitter about Eddington's conduct and the outcome of the controversy.<sup>111</sup> We can rationalise this by arguing that Chandrasekhar's articles on Eddington were written much later, post-1970s, and with his award of the Nobel Prize, Chandrasekhar could be generous about Eddington, and that we cannot compare his comments over a gap of twenty years. But when McNally was in Chicago, a period of over 25 years had already passed since the fateful RAS meeting in January 1935. It would seem as though Chandrasekhar's public and private persona, as we have already seen with Milne, was entirely different. He managed to maintain a public face in which he did not seem to have any regret or anger towards Eddington, and there is no doubt that Chandrasekhar held Eddington in very high regard, but we cannot completely deny that the impact of the controversy was enormous and, inevitably, influenced Chandrasekhar's career path.<sup>112</sup> If Eddington had not caused and prolonged the controversy, Chandrasekhar's contribution to astrophysics would have been openly recognised thirty years earlier than what actually occurred. This must have rankled.

It is interesting that the older generation of scientists, particularly McCrea, who had access to Eddington and his peer group, are adamant that there were no ill feeling between the two astrophysicists. Many maintained and refused to acknowledge that Chandrasekhar would have said anything negative about Eddington. Regarding the

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<sup>111</sup> Private communication with McNally at the RAS, Eddington Memorial Meeting, 12 March 2004, London.

<sup>112</sup> Chandrasekhar's regard for Eddington is very apparent from the letters he sent to his father and his personal notes. Chandrasekhar Archive.

controversy, Lalitha Chandrasekhar's criticism of Eddington can be seen in the passage below,

'It was devastating! Why couldn't Eddington have given a warning to Chandra when he periodically came over to Chandra's rooms at Trinity College to see how his calculations were coming along? He could have said "Look here, Chandrasekhar, I do not agree with your conclusions. In fact, I am going to oppose them at the next R.A.S. meeting where you are going to present your paper." That would have been sportsmanlike. But he kept it a secret and attacked without warning.'<sup>113</sup>

She only goes as far as saying that Eddington's conduct was 'unsportsmanlike', a very generous criticism considering the impact of the controversy on Chandrasekhar. In the remainder of the article she acknowledges Eddington's influence in probably creating in Chandrasekhar's scientific persona and the way he conducted his research.<sup>114</sup> There is a strong sense of a love/hate relationship in which even as Chandrasekhar finds himself at the receiving end of a great betrayal, he is still bound to and in awe of Eddington. It is only the scientists of later generations who had never met Eddington who are more open to the idea that Chandrasekhar could have harboured a grievance against Eddington. In fact, they think it is highly likely.<sup>115</sup>

It seems likely that those who experienced, and were a part of, the scientific atmosphere of Cambridge in the inter-war period, and who were surrounded by the great figures of British science at Oxbridge want to maintain that idyllic innocence of science which was so rudely shattered by the Second World War and the subsequent change in science since. The advent of Big Science changed the scientific world to the extent that the golden era of science for science's sake seemed to have been only a distant dream.

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<sup>113</sup> Wald (1998): 278.

<sup>114</sup> Lalitha Chandrasekhar interview, 1998.

<sup>115</sup> Almost all the astronomers interviewed with the sole of exception of McCrea believed Chandrasekhar never fully recovered from his controversy with Eddington. Publicly Chandrasekhar never portrayed

Naturally the way that science, especially astronomy and astrophysics, was conducted has changed considerably. There are a lot more collaboration in published papers compared to the enormous single output by Eddington, Jeans and Milne which characterised astrophysics and astronomy in the inter-war years. There was little published collaboration, although the collegiate atmosphere and high table dinners would have led to several discussions between scientists which, although no material evidence remains, are sometimes noted down in various biographical anecdotes.<sup>116</sup>

In summary, Chandrasekhar found the controversy to be devastating and immediately tried to garner support for this theory, going straight to the quantum physicists rather than the astronomers in order to validate relativistic degeneracy. From the beginning Bohr, Dirac, Pauli and Rosenfeld all thought Eddington's reasoning was confused and wrong, and although they agreed with Chandrasekhar, he was unable to gather any public support from them. The astronomers took longer but were eventually convinced by 1939 when Chandrasekhar had his final meeting with Eddington in the International Conference on Novae and White Dwarfs. Once again the support was acknowledged privately rather than publicly, although there was a significant change in the way Chandrasekhar's theory was treated at the meeting. The intricacy involved in the controversy and its aftermath show Chandrasekhar's growth not only as a scientist but as a person who needs to be aware of the politics surrounding academia in order to survive in his career. We have seen the side-taking that goes with scientific debate not only in the Chandrasekhar-Eddington controversy, but from earlier with the Eddington-

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Eddington in a negative light, yet in private, many of his students and colleagues recall his anger at his treatment.

<sup>116</sup> Kilmister (1994) notes that there are several discussions between Eddington and George Howard Darwin regarding Dirac's relativistic equation for the electron.

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Jeans-Milne controversies which formed the historical and scientific background to this case.

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## CHAPTER FIVE: Eddington's Arguments

In this chapter, I aim to analyse the reasons behind Eddington's behaviour in the controversy. It is not easy to give one definitive reason: his motives are complex and I will try to address the various issues which ultimately led him to reject relativistic degeneracy and the notion of gravitational collapse and to show that the roots lie as far back as 1916 when singularities were first introduced mathematically in relativity physics. In addition to his scientific standing regarding Chandrasekhar's theory, I will also assess his gradual distancing from straight astrophysics and growing involvement in his more philosophical fundamental theory. As Eddington's fundamental theory is a work of extreme complexity and, as many have said, very obscure with its distortion and 'idiosyncratic interpretation' of quantum mechanics and relativity, it is beyond the scope of this thesis to fully discuss the theory. I will, however, touch on some of the issues which are directly relevant to this thesis: those that may explain Eddington's rejection of relativistic degeneracy and singularities.

The notion of relativistic degeneracy, the limiting mass and singularities are intimately connected and, Eddington believed, cannot be separated. As Chandrasekhar had shown, relativistic degeneracy will always produce a singularity as one of its conclusions. However, the limiting mass alone is not what Eddington opposes. By acknowledging the existence of a limiting mass, Eddington will have to accept the possible existence of gravitational collapse to which stars over the limiting mass will eventually succumb. The ultimate point of opposition is, therefore, the idea of gravitational collapse and all the physical and philosophical implications that this entails.

So why does Eddington abhor the idea of gravitational collapse to such an extent? There have been a number of significant papers that have been published which

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discuss the possibility of singularities before and after Einstein's publication of general relativity, but both Eddington and Einstein reject the conclusions which these papers draw on the possible existence of singularities. Chandrasekhar's research into the limiting mass of white dwarf stars dug deeply into Eddington's views on singularities which he thought had been successfully resolved by Fowler's introduction of electron degeneracy. We recall that Fowler's use of electron degeneracy successfully introduced a balance against the gravitational force which otherwise would force the white dwarf to keep on contracting indefinitely without having the energy to do so (Eddington's paradox).

Over the intervening years, there have only been a few serious discussions by Clive Kilmister and Werner Israel regarding Eddington's motives behind his opposition to relativistic degeneracy and his controversy with Chandrasekhar. The other published discussions are brief and focus mainly on Eddington's behaviour, such as jealousy and philosophical obscurity, without speculating on his scientific motives. As Eddington did not leave behind any notes or scientific diaries, all we have to work with are his published material. I will discuss the literature on Eddington's motives and will put forward a number of explanations which may answer why Eddington opposed the limiting mass.

My view is that his rejection of relativistic degeneracy stemmed from the wider issue of the limiting mass and what it implied to physics and the universe. By accepting the limiting mass, Eddington would have had to accept the possible existence of singularities. By attacking relativistic degeneracy Eddington was attacking the root of the problem. Without relativistic degeneracy, there will be no limiting mass and hence no definite possibility of singularities. As we shall see, this was not a problem he was facing for the first time. The idea of singularities has been around in physics and



astronomy for a number of years. As speculation, it has been hovering around in the scientific conscience for several hundred years. But it had not been dealt with mathematically until the formulation of general relativity by Einstein in 1915. General relativity introduced a new way of thinking about cosmology and from 1917, with the introduction of the Einstein and de Sitter universes which were static models and did not change with time, a new field of research emerged: relativistic cosmology. But until Chandrasekhar's theory, singularities were seen only as mathematical artefacts. The limiting mass of white dwarfs showed that it was possible for singularities to physically exist. I will try to show that Eddington's continuous attack on relativistic degeneracy was part of a *sustained campaign* against singularities which remained constant and never wavered throughout his career.

Eddington was the leading authority on general relativity in Britain during the 1920s and 1930s. Apart from disseminating the theory within Cambridge, through his theoretical exposition of general relativity in scientific articles and lectures, and the English speaking world, through his popular articles and books, he kept abreast of all the emerging research in the new theory. Einstein's foray into cosmology did not go unnoticed and Eddington soon became involved in cosmological research, to which he made frequent contributions. Together with his student George Cunliffe McVittie, Eddington was researching the stability of the Einstein and de Sitter models throughout the 1920s.<sup>1</sup>

In 1930, Georges Lemaître, the Belgian astronomer, mathematician and priest, introduced the first model of an evolutionary universe. The non-static model of the universe was expanding with time. As well as projecting the expansion of the universe into the future, it was also possible to extrapolate backwards in time towards the

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<sup>1</sup> McVittie (1967): 295; Smith (1982): 174, 187; Kerszberg (1989): 336, 350; McCrea (1990): 55.

beginning of the universe. The creation of the universe implied the possible existence of a singularity. Cosmology, as Einstein, de Sitter and Eddington knew it, changed forever.

As well as working in astrophysics and cosmology in the late 1920s and early 1930s, Eddington had also embarked on a new enterprise. This was his *primum mobile*: bridging the gap between general relativity and quantum mechanics. In 1928 the Cambridge quantum physicist Paul Dirac formulated and published his relativistic wave equation of the electron. Dirac, who was only twenty six at the time, attempted to combine relativity with wave mechanics but without the use of tensors. This had never been done before and was a powerful shock to Eddington, who spent the rest of his career trying to complete what Dirac began, but in the language of relativity: tensor mechanics. His move to try and unify general relativity and quantum mechanics quickly turned into a full blown investigation into the fundamental constants of the universe, especially the fine structure constant, and later became known as his fundamental theory.

To understand Eddington's rejection of singularities, we would need to look at all three areas of research that occupied him during this period. These are, in broad terms, the following:

- 1) Cosmology.
- 2) Bridging general relativity and quantum mechanics: Dirac's equation and the fine structure constant.
- 3) Astrophysics and relativistic degeneracy.

I will assess the impact of each area on Eddington and his work and will discuss the way they may have affected Eddington's views on singularities and hence his reaction to Chandrasekhar's theory. I will also discuss whether Eddington's religious beliefs, which were an integral part of his life, had any impact on his scientific views.

## 5.1 Cosmology

### 5.1.1 The Static Universe and the Einstein and de Sitter Models

In 1917 Einstein published a paper using his new theory of general relativity to describe the first relativistic model of the entire universe.<sup>2</sup> Cosmology and the nature of the universe before 1917 were very different from that which is understood today: it was mainly restricted to our solar system, and later, to our galaxy, the Milky Way. All objects that could be observed in the sky were thought to have belonged within our galaxy.<sup>3</sup> It was not until Einstein had formulated his general theory of relativity that models of the universe that extended beyond our galaxy were first constructed. It is similar to a paradigm shift in our view of the universe and led to the birth of modern cosmology.

The Newtonian model of the universe was a spatially infinite universe with an infinite number of stars. In this model, it was extremely difficult for astronomers to define the gravitational force acting upon a body in any definite way. To solve this problem, two German theoreticians, Carl von Neumann and Hugo Seeliger, first suggested in the mid-1890s that the model be modified so that the spatially infinite universe contained a finite amount of matter. This gave a measurable gravitational force for the universe but also predicted a collapse of the universe due to this gravitational force.<sup>4</sup>

From his own writing and the various historical literature on Einstein's cosmology, it is clear that what most influenced Einstein's cosmology were the theories

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<sup>2</sup> Kragh (1987): 115, Kragh (1996): 8, 403. The paper is Einstein, A. (1917), 'Kosmologische Betrachtungen zur allgemeinen Relativitätstheorie,' *Sitzungsberichte der königliche Preussische Akademie der Wissenschaften zu Berlin*: 142-152. An English translation can be found in Einstein, A. et al. (1923), *The Principle of Relativity*, (New York: Dover): 175-78.

<sup>3</sup> Kragh (1996): 3, 5.

of the physicist and philosopher Ernst Mach. Mach's law of inertia described a universe in which a system on which no force acts is at rest or in uniform motion relative, not to absolute space (like Newton's), but, to the fixed stars idealized as a rigid system. Thus with heavy bodies at large distances, the relative motion averages out to zero. This Einstein understood as 'the total inertia of a mass point is an effect due to the presence of all other masses.' Matter is therefore influenced by other matter. This was Mach's principle, and Einstein created a theory of gravity that showed this.<sup>5</sup>

In 1915 Einstein formulated his field equations to be in agreement with the Newtonian law of gravitation for weak fields of the form

$$R_{mn} - \frac{1}{2} g_{mn} R = -\kappa T_{mn}$$

which relates the geometrical nature (left hand side of the equation) to the physical nature (right hand side of the equation) of the universe. A consequence of the field equations was that the space-time continuum would be curved and that a constant density  $\rho$  would give an isotropic, homogeneous universe.<sup>6</sup>

There are ten equations for ten unknown  $g_{mn}$ , the metric tensor (which specifies the gravitational field and the scale of time and space intervals),  $\kappa$  is a constant and  $T_{mn}$  is the energy-momentum (or energy-stress) tensor which represents various sources of energy and momentum including pressure and electrical charges.  $R_{mn}$  is the Ricci tensor which is obtained from the Riemann-Christoffel tensor  $R^p_{qrs}$  (a tensor constructed solely

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<sup>4</sup> Kragh (1987): 114; Kragh (1996): 6.

<sup>5</sup> Eddington (1920): 164; Pais (1982): 284-285; Smith (1982): 170.

<sup>6</sup> An isotropic, homogeneous universe is one in which matter is spread uniformly in all directions.

from the components of the metric tensor and its first and second derivatives with respect to the co-ordinates).<sup>7</sup>

Einstein also tried to tackle the problem of the Newtonian universe by introducing the concept of a finite amount of matter. But he also made his model a spatially closed one: a universe that was spatially finite, static and contained a finite amount of matter. Even with these modifications, Einstein found that the universe was not static, it will contract with time due to the gravitational force. Einstein's law of gravitation,  $G_{mn} = 0$ , just like Newtonian attraction between material objects where attraction varies as the inverse square of their distance apart, should imply that for the motions of spiral galaxies, the tendency should be for matter to fall together. Instead, what we see is them running away from each other. Einstein then faced a problem where his law did not work: infinity, so 'he abolished infinity. He slightly altered his equations so as to make space at great distances bend round until it closed up.' If you continue in an Einstein universe, you never reach infinity, you find yourself at the starting point again. In order to do this he amended his law of gravitation to  $G_{mn} = \Lambda g_{mn}$  by adding the cosmological constant  $\Lambda$ .<sup>8</sup>

In 1917, to address this problem, he introduced the cosmological constant  $\Lambda$  and modified the form of his field equations to

$$R_{mn} - \frac{1}{2} g_{mn} R - \Lambda g_{mn} = -\kappa T_{mn}$$

<sup>7</sup> Smith (1982): 169; Kragh (1996): 8-9; Pais (1982): 285-287.

<sup>8</sup> Eddington (1920): 140-141; Eddington (1923): 82, 152-155; Eddington (1933.1958): 23. Eddington uses the symbol  $\lambda$  for the cosmological constant instead of  $\Lambda$ .

By introducing the cosmological constant  $\Lambda$  as a repulsive force balancing the gravitational force of matter, Einstein was able to create a model in static equilibrium: static in the sense that the curvature of the universe was independent of time.

As the basis of the model of the Universe that he expounded in his 1917 paper, Einstein assumed a uniform distribution of matter in static equilibrium. Einstein's motive for considering such a model was that he was deeply concerned about the boundary conditions he should impose on his field equations. Einstein argued in his general theory that as a consequence of the gravitational fields in the Universe the space-time continuum was 'curved'. Although he wrote that the curvature is variable in time and space, he claimed that he could roughly approximate the actual curvature by means of a spherical space. He thus decided not to solve the boundary value problem, but rather to dissolve it 'by regarding the Universe as a continuum closed with respect to its spatial dimensions'.<sup>9</sup>

What Einstein did was to reshape the universe so that it was no longer infinite. But the question of boundary conditions was problematic and in order to solve that, Einstein simply removed the problem itself. Einstein's universe was such that the density of matter determined the radius of curvature in the universe. He envisaged a universe that had no boundaries, but was still finite. He took infinity itself out of the problem. It was a big conceptual shift from Newton's infinite universe to Einstein's bounded and spatially closed universe.<sup>10</sup>

But the cosmological constant needed to be very small, 'a term which introduces a cosmic repulsion proportional to the distance, negligible at small distances but increasingly important at very large distances.'<sup>11</sup>

The effect of the  $\Lambda$  term was to produce a repulsive field to oppose the gravitational field; without its presence Einstein calculated that the stars could not remain in equilibrium, to him an unacceptable conclusion. The size of the  $\Lambda$  term, moreover, defined the mean density of matter as well as the volume of the Universe. Einstein

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<sup>9</sup> Smith (1982): 169.

<sup>10</sup> Eddington (1933/1958): 35; Eddington (1935c/1955): 87. The Einstein universe was a closed universe without boundaries i.e. the surface area is finite but we never come to a boundary, but we can never be more than a limited distance away from our starting point e.g. a spherical surface in two dimensional space.

<sup>11</sup> Kragh (1987): 115; Kragh (1996): 9.

quickly seems to have doubted his decision to employ the  $\Lambda$  term. Certainly he felt that it impaired the simplicity and elegance he believed all fundamental physical equations should possess, and in 1919 he remarked that he hoped soon to expunge it from his field equations.<sup>12</sup>

Einstein's universe, however, was only stable when matter was present and the density was constant. When density is zero, the system breaks down. But in 1917, the same year, Willem de Sitter, professor of astronomy at the University of Leiden and director of the Leiden Observatory, found a solution for when density is zero: a static universe that was empty. De Sitter named Einstein's universe Solution A and his own Solution B.

De Sitter was one of the few theoreticians who were interested in Einstein's theories from the beginning, showing great interest in special relativity since its formulation in 1905, and quickly realised the important astronomical and cosmological implications of Einstein's general relativity. As we have seen earlier, because of Holland's neutral status during the First World War, he was also instrumental in transporting and disseminating the theory of general relativity, via Eddington, outside Germany to the rest of Europe during this period.<sup>13</sup> De Sitter prepared a series of reports for the RAS in which he gave details of Einstein's new theory. It was in his third paper on general relativity, presented to the RAS in London in 1917 by Eddington, who was then Secretary of the society, that de Sitter first outlined his cosmological model.<sup>14</sup>

He had found a solution that contradicted Mach's principle and showed an empty universe which 'by virtue of having nothing in it to move, was also, like Einstein's model, static. De Sitter had further found that if a particle were introduced

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<sup>12</sup> Smith (1982): 170. Einstein's struggle with the cosmological is also discussed in Kragh (1996), Kerszberg (1989), Pais (1982).

<sup>13</sup> Earman and Glymour (1980); Warwick (2003); Stanley (2003); Douglas (1956); Chandrasekhar (1987): 110.

<sup>14</sup> Kragh (1996): 11.

into his empty universe it would behave as if it possessed inertia, in violent and obvious contradiction to Mach's Principle'. This was a problem for Einstein, whose conception of Mach's Principle was that the curved space-time was generated by matter, because de Sitter's universe did not contain matter.<sup>15</sup> As Eddington so succinctly put it, 'the situation has been summed up in the statement that Einstein's universe contains matter but no motion and de Sitter's contains motion but no matter.'<sup>16</sup> As long as no matter was introduced into the system, it was in equilibrium. In Eddington's words, 'De Sitter's is also reckoned technically as an equilibrium solution, but it is a bit of a fraud; being entirely empty, there is nothing in his world whose equilibrium could possibly be upset.'<sup>17</sup> If a particle was introduced, however, it would appear to move away from the observer, accelerating outwards and de Sitter predicted there would be an observed redshift. The larger the distance from the observer, the larger the acceleration and redshift causing the equilibrium of the system to break down.

In Einstein's solution there was no systematic redshift. In de Sitter's model, because matter will be accelerating away from the observer, there should be a noticeable redshift for very distant objects.<sup>18</sup> This redshift, de Sitter argued, was an intrinsic property of space and time in his solution, 'a slowing down of distant atomic vibrations caused by the structure of spacetime', and not due to the real recession of distant stars and nebulae.<sup>19</sup> De Sitter did not see his model as a universe that is expanding; it is still a

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<sup>15</sup> Kragh (1996): 12.

<sup>16</sup> Eddington (1933/1958): 46. De Sitter himself describes the two solutions as 'Einstein's solution gives a world full of matter, but no motion; mine gives a world full of motion, but no matter.' in *Proceedings of the RAS Meetings* (1930b): 163.

<sup>17</sup> *Proceedings of the RAS Meetings* (1930b): 162.

<sup>18</sup> Kragh and Smith (2003): 145.

<sup>19</sup> Eddington (1923b): 161; Eddington (1933/1958): 49; Kragh (1987): 116. The redshift predicted in de Sitter's universe is that due to the Doppler Effect. When a star is moving away from the observer, the light it is emitting will be stretched and its wavelength will increase. This will cause the light to shift towards the red end of the optical spectrum.



static universe.<sup>20</sup> Measuring the predicted galactic redshifts would provide a test for his solution: it would show whether the stars were accelerating away from us. If de Sitter's model was a true approximation of the universe, then a relation between redshift and distance was to be expected: the larger the redshift, the greater the distance of the celestial object.

Eddington, who had been following Einstein and de Sitter's cosmological research closely, at first objected to Einstein's solution because it was reminiscent of the aether as there was a constant amount of matter permeating the universe. He championed de Sitter's solution because it predicted redshift. As soon as matter was introduced it scattered, and that 'this property is perhaps rather in favour of de Sitter's theory than against it.' But by 1923 he switched his allegiance to Einstein's model because 'he perceived in Einstein's solution the chance of linking the ratio of the electron's radius and mass to the number of particles in the Universe, and for him this outweighed the advantages of de Sitter's solution.'<sup>21</sup>

Astronomers have been trying to measure galactic redshifts since 1914 when Vesto Slipher, working at the Lowell Observatory in the States, discovered that spiral nebulae exhibited spectral shifts towards both the red and blue ends of their spectrum.<sup>22</sup> The research conducted by Slipher and others showed a correlation between the redshift of light from nebulae and their distance. As redshifts were considered a result of the Doppler effect, from de Sitter's theory, this indicated a velocity-distance relation. Calculating the distances of spiral nebulae and novae was a notoriously inaccurate endeavour. Astronomers found the apparent luminosities of extragalactic spiral nebulae,

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<sup>20</sup> Smith (1982): 172.

<sup>21</sup> Smith (1982): 174; Eddington (1923*b*): 161; Kragh (1987): 118.

<sup>22</sup> Just as in the case of redshifts, alternatively, for a star moving towards the observer, the light will be squeezed forward, its wavelength shortened and there will be an observed blueshift.

necessary to calculate distances, difficult to measure accurately.<sup>23</sup> The publicity received by de Sitter's universe and its prediction of redshift (mainly due to Eddington's support), increased the popularity of cosmological research and the search was on to find this redshift-distance relation.

In 1923, Hermann Weyl, the German mathematician and physicist, combined de Sitter's solution with his (Weyl's) principle which states that stars lie on a pencil of geodesics that diverge from a common event in the past. This strongly indicated a linear relation between redshift and distance.<sup>24</sup> Astronomers knew there was some kind of a redshift-distance relation from de Sitter's model, and Weyl's hypothesis provided an insight into the type of relation that might exist.

In 1924, Ludwik Silberstein claimed that he had found a linear relation between redshift and distance in the form

$$d\lambda/\lambda = \pm r/R$$

where  $r$  is the distance of the luminous source and  $R$  the curvature of the radius of the universe. The double sign indicates that the formula is valid for blueshifts as well as redshifts.

However, many astronomers felt that Silberstein was selective of his data and had ignored data which did not agree with his prediction. His method was ridiculed and what quickly became known as the 'Silberstein effect' tarnished his reputation as well as diminishing the importance and objectivity of redshift research. Because of this, any research on the redshift-distance relation was regarded with scepticism and many were

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<sup>23</sup> Gribbin (1996): 343; Kragh (1987): 116; Kragh and Smith (2003): 144.

<sup>24</sup> Smith (1982): 179.

put off finding a relation until there was substantial good quality data and reliable measurement to support their theories.<sup>25</sup>

In 1925 Edwin Hubble of the Mount Wilson Observatory in the States announced his discovery of Cepheid variables in spiral galaxies. Cepheid variables are pulsating stars whose periodic variations are directly related to their luminosities like standard candles.<sup>26</sup> Up until that point, astronomers had to rely on the crude distance measurements provided by spiral galaxies. Now it was possible to make accurate measurements of the distances of these galaxies using the period-luminosity relation of the Cepheid variables.

Using this information, Georges Lemaître, at the University of Louvain, derived a linear redshift-distance relation, supported by observational evidence in 1925. Lemaître was unhappy with de Sitter's model because it introduced a centre to the universe. Instead he derived a model in which the radius of curvature depended on time, a solution which is non-static. Thus the observed redshift would be

$$d\lambda/\lambda = -r/t_0$$

where  $r$  is the distance between the light source and observer and  $t_0$  the time measured by the observer. This is similar to Silberstein's formula but allowing only for redshifts (hence the minus sign). But to get rid of the centre of the universe, Lemaître had to make his model non-static and without curvature, 'de Sitter's solution has to be abandoned not because it is non-static, but because it does not give a finite space without introducing an impossible boundary.' This he found difficult to accept and

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<sup>25</sup> Kragh (1996): 15.

abandoned his model as neither Einstein's nor de Sitter's model seemed adequate. He needed something in between the two models and in 1925 began to realise that such a solution would imply an expanding universe.<sup>27</sup> Kragh states that Lemaître 'seems at the time vaguely to have recognised the possibility of a third alternative, the expanding universe, but only discussed this explicitly two years later.' In 1928, Howard Percy Robertson at Princeton also found that the measured redshift corresponded to his independent derivation of the redshift-distance relation. Both derivations were solutions to a *non-static* de Sitter universe.<sup>28</sup>

In the same year, Hubble, together with Milton Lasell Humason, began a redshift survey, extending and reinterpreting the research programme begun by Slipher a decade earlier at the Mount Wilson Observatory. In 1929 Hubble discovered the linear redshift-distance relation empirically. It was now possible to measure the whole of the observable universe. This became known as Hubble's Law.<sup>29</sup>

By the late 1920s, many theoretical cosmologists were becoming increasingly unhappy with the static model of the universe, even though de Sitter's prediction of the linear redshift-distance relation had been proved correct. The main problem was with the static nature of his, and Einstein's, universe. The consensus was that neither models worked and that another way had to be found to correctly describe the universe.

### 5.1.2 The Expanding Universe and the Lemaître-Friedmann Model

In 1925, Lemaître first formulated his solutions to Einstein's field equations which described a non-static de Sitter universe using observational evidence he obtained

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<sup>26</sup> Eddington (1933/1958): 7; Kragh (1996): 17. The period-luminosity relation of Cepheids variables was discovered by Henrietta Swann Leavitt at Harvard College Observatory in 1912. She found that the slower the star goes through its cycle of variation, the brighter the star.

<sup>27</sup> Kragh (1987): 119.

<sup>28</sup> Kragh (1996): 15.

from Slipher to support his theory. Although he abandoned the theory at the time, he returned to it two years later in 1927 when he published three papers on relativistic cosmology in *Annales de la Société Scientifique de Bruxelles*, a local journal of science.<sup>30</sup> Lemaître's aim was to construct a theory of a physically real universe by combining the theoretical model he had formulated with observational results. We can see that Lemaître was not just interested in a mathematical construction, as in the case of Einstein and de Sitter, because he made use of the available redshift data. Previously Einstein and de Sitter had only constructed mathematical models of the universe. Although de Sitter's model predicted the redshift-distance relation, his was essentially a mathematical, not a physical universe as it did not contain matter and was only stable when empty.

Born in 1894 in Charleroi, Belgium, Lemaître studied engineering at the University of Louvain until he joined the Belgian army to fight in the First World War. He decided to become a priest at the end of the war and changed his engineering degree to that in mathematical sciences, receiving a doctorate in 1920. He soon became interested in general relativity and submitted three essays for a scholarship in 1923. In the same year, he was ordained a priest and was awarded a scholarship to study with Eddington at Cambridge on the topic of simultaneity in general relativity. The following year he went to Harvard to work with Harlow Shapley on relativistic cosmology and, at the same time, he enrolled at the Massachusetts Institute of Technology where Hubble was based at the Mount Wilson Observatory to do a doctoral dissertation which he

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<sup>29</sup> Kragh (1996): 16; Kragh and Smith (2003): 154. Hubble's Law was also referred to as the Hubble-Humason Law but Humason's name was later dropped.

<sup>30</sup> Godart (1992): 440.

completed in 1926, but never published. He later returned to the University of Louvain as associate professor in the mathematics department.<sup>31</sup>

Lemaître found that his reworking of Einstein's field equations revealed a steady expansion of the universe. This predicted expansion gives a redshift for distant matter which was supported by observational evidence. Lemaître's work received little recognition except from Einstein who commented on it when they met at the Solvay conference in 1927. Lemaître was not invited to the conference itself, but managed to talk to Einstein about his papers. Lemaître's model showed that the static Einstein solution was unstable and that the universe was not only non-static but expanding. This expansion naturally tended towards zero when reversed in time. This, to Einstein and de Sitter, was physically intolerable. In a draft of a letter Lemaître had written to Eddington he recalls that Einstein had seen his paper and agreed with the mathematics but he thought the physics was 'tout à fait abominable'.<sup>32</sup> It is also at this meeting that Einstein first informs Lemaître of Alexander A. Friedmann's paper on non-static solutions to Einstein's field equations that was published in *Zeitschrift für Physik* in 1922. Lemaître's solution is the same as Friedmann's, but he claimed he did not know of Friedmann's papers; he only learnt of them when Einstein told him in 1927 (six months after the publication of his own paper).<sup>33</sup>

Born in 1888, Friedmann was one of the foremost theoretical physicists working at the Main Geophysical Observatory in St. Petersburg in the 1920s.<sup>34</sup> Friedmann's solutions were the first models of a non-static universe, one that changes with time,

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<sup>31</sup> Godart (1992): 437.

<sup>32</sup> Eisenstaedt, J (1993): 354; Kragh and Smith (2003): 148; Kragh (1987): 125; Kragh (1996): 31-32; Godart (1992): 442. Godart, Lemaître's collaborator and biographer, writes, 'Einstein was most abrupt: "Your calculations are correct, but your physical insight is abominable."'

<sup>33</sup> Kragh and Smith (2003): 147. Lemaître says the same to de Sitter and Eddington in 1930.

<sup>34</sup> Kragh (1996): 23. Also see Tropp, E.A., Frenkal, V.Ya. and Chernin, A.D (1993), *Alexander A. Friedmann: The Man who made the Universe Expand* (Cambridge: Cambridge University Press) for a full biography.

since the formulation of the Einstein and de Sitter universes. His work was primarily mathematical and did not incorporate any astronomical observations to support it – in fact there was no mention of redshifts at all. Friedmann's universe was cyclical and this implied the existence of singularities, but he did not develop or emphasise singularities apart from stating that they *may* occur in his models. Kragh says that singularities were 'at the time ...unwelcome ... unthinkable ... and seen as a blemish.' This was also the case for Cornelias Lanczos at the University of Freiburg who published similar results in 1923.<sup>35</sup> Kragh writes,

Even that was regarded as a blemish at a time when a universe expanding from a singularity was almost unthinkable. In 1924 Weyl argued that his own version of cosmology "has the great advantage [over Lanczos's] of not introducing a singular initial moment, of conserving the homogeneousness of time."<sup>36</sup>

Friedmann himself, though he explicitly said that one of the solutions was an expanding model, did not emphasise that the universe was expanding. To him, and also to the few that read his papers, it was a mathematical game. Astronomers were mainly unaware of his work because it made no connections with astronomical observations.

Einstein was unhappy with Friedmann's solutions, especially the prediction of inherent singularities in the universe, and when Friedmann published his results, Einstein wrote a short note to the *Zeitschrift* proving Friedmann's results were wrong. But it was Einstein's proof that was wrong and he later amended his note accordingly.<sup>37</sup>

Friedmann's results demonstrated a non-static universe that was either a homogeneously expanding or cyclical universe. But his was an essentially mathematical theory with no relevance to physical reality and no connection to astronomical

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<sup>35</sup> Kragh (1987): 118; Eisenstaedt (1993): 361.

<sup>36</sup> Kragh (1987): 122.

<sup>37</sup> Kerszberg (1989): 13; Kragh (1996): 23-27; Hoffmann (1972): 215-219; Brian (1996): 194.

observations. It introduced two concepts to relativistic cosmology: the age of the world and the creation of the world, but he did not really attach much importance to either.

Friedmann's work, apart from the initial comment by Einstein in 1922 and a number of citations, was never fully acknowledged nor taken up by mathematicians or physicists until Lemaître 'rediscovered' his work in the 1930s.<sup>38</sup> Lemaître's 1927 paper was published in a local scientific journal, but Friedmann's was published in Germany's leading physics journal and the lack of response is puzzling. Kerszberg lists a number of reasons: the mathematics was of too high a level, results lacked astronomical relevance, no one realised that it was an alternative cosmological model and Einstein's criticism, although Kerszberg believes this should have heightened interest in Friedmann's model.<sup>39</sup> Perhaps Friedmann's early death in 1925 at the age of 36 may have also been a contributing factor.

Lemaître's solution was essentially similar to that of Friedmann's except in two respects: he introduced radiation pressure to the theory and throughout his paper he referred to astronomical observations to support his claims. He wanted to create a physically realistic cosmology, not just a mathematical model. He needed an explanation for this expanding universe and tentatively thought it was due to radiation. Lemaître also explicitly connected his theory with that of the recession of nebulae in which he derived the Doppler shift which showed the proportionality of the velocity of receding galaxies and their distances from earth.<sup>40</sup>

Apart from Lemaître's meeting with Einstein, there is a complete silence regarding Lemaître's expanding universe. Kragh is puzzled by the lack of response to Lemaître's paper. It is understandable that many were not aware of the paper due to its

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<sup>38</sup> Eddington (1933/1958): 46.

<sup>39</sup> Kerszberg (1989): 13.

<sup>40</sup> Kragh (1987): 123-124.



publication in a relatively obscure scientific journal, yet Lemaître had worked and been in touch with almost all the prominent astronomers and cosmologists who were active in this field at Cambridge, Harvard and MIT.<sup>41</sup> Eisenstaedt believes that the lack of interest may have been due to the 'fashion' in physics at the time. It was not until the 1930s that cosmology really became popular as a potential field of research. Until then, physicists were generally uninterested in relativistic cosmology, and relativity was still considered an obscure and difficult subject.

By 1930, de Sitter and Eddington had come to the conclusion that cosmology had to diverge from the static models of Einstein and de Sitter, with Eddington asking, 'One puzzling question is why there should be only two solutions. I suppose the trouble is that people look for static solutions. Solution A is such a static solution. Solution B is, on the contrary, non-static and expanding, but as there isn't any matter in it that doesn't matter.' This was reported in the *Observatory* which upon reading, Lemaître immediately contacted Eddington to remind him that he had already solved the problem and had sent a copy of his 1927 paper to him.<sup>42</sup> Eddington, who had apparently forgotten about Lemaître's paper, immediately sent a copy to de Sitter and communicated an English translation of Lemaître's paper to the RAS.<sup>43</sup> With the backing and authority of Eddington and de Sitter, Lemaître's model of the expanding universe soon became widely known amongst astronomers and cosmologists and almost all research in cosmology was concentrated on it. The theory's popularity received an

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<sup>41</sup> Eisenstaedt (1993): 379. Until then, physicists were generally uninterested in relativistic cosmology, and relativity was still thought of as a difficult subject.

<sup>42</sup> Smith (1979): 154; Kragh (1987): 126; Godart (1992): 442-443; Eisenstaedt (1993): 361. The article Lemaître read about the discussion between Eddington and de Sitter at the Royal Astronomical Society is published in *Observatory* 53: 38-39.

<sup>43</sup> Proceedings of the RAS Meetings (1930b). Kragh (1996): 405; McVittie (1967); McVittie OH322 (1978). Eddington had apparently forgotten about Lemaître's paper, although he does not mention this slip in the RAS discussion reported in *Observatory*, merely stating that he had 'learnt of a remarkable paper by Abbé G. Lemaître, of Louvain, published in 1927.' Nor does he mention it in any of his other papers or monographs. However, Eddington's research student McVittie recalled Eddington's words regarding

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additional boost with the publication of several popular books by Eddington, Jeans and de Sitter.

It was only with the increased coverage of Lemaître's (and hence Friedmann's) theory that the importance of the expanding universe came to be understood. Together with Hubble's discovery of the redshift-distance law, the expanding universe sparked a paradigm shift in cosmological theory.

Eddington had always been a strong advocate of the expanding universe, favouring a model in which the mass of the universe equalled that of the Einstein universe. But he, unlike Lemaître, was averse to the idea of a beginning to the universe and his preferred model which was an extension of Lemaître's solution began with an already existing pre-universe, the static Einstein universe. Starting with the Einstein universe, the expanding model then progresses until it reaches the de Sitter universe when the density of celestial material approaches zero and there is no more matter to expand. De Sitter's model is therefore the limiting version of the expanding universe.<sup>44</sup> The Lemaître-Eddington model was essentially a 'world without a proper beginning'.<sup>45</sup>

But Lemaître soon began to question how the expansion may have started. This had already been considered by Eddington and Richard Tolman at the California Institute of Technology. Tolman advocated the annihilation of matter into radiation to explain the recession of the nebulae. Eddington suggested instead that the formation of condensations in the Einstein world may have been the catalyst for expansion to commence. Lemaître took Eddington's stance and created a theory of 'stagnation', a

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Lemaître's 1927 paper as, 'I'm sure Lemaître must have sent me a reprint, he's just sent me another, but I'd forgotten about it.'

<sup>44</sup> Proceedings of the RAS Meetings (1930*b*): 162; Eddington (1933/1958): 45-47.

<sup>45</sup> Kragh (1996): 45.

condensation process in which the total pressure diminishes, thus increasing the radius which leads to expansion.<sup>46</sup>

In 1931 Eddington gave a talk to the British Mathematical Association, which was later published in *Nature* with the title 'The End of the World: from the Standpoint of Mathematical Physics', in which he discusses entropy as time's arrow and the heat death of the universe. He also considers the state of the universe if time was traced backwards when entropy becomes zero asking whether this would correspond to the beginning of the world. Eddington replied that this question 'probably laid outside the range of scientific reasoning, but that "philosophically the notion of a beginning of Nature is repugnant to me"'.<sup>47</sup>

A few weeks after reading Eddington's paper, Lemaître replied with a brief paper in *Nature* titled, 'The Beginning of the World from the Point of View of Quantum Theory' in which he began to weave together his idea of the primeval atom from which the world began to divide and expand. Lemaître writes,

If the world has begun with a simple quantum, the notions of space and time would altogether fail to have any meaning at the beginning; they would only begin to have a sensible meaning when the original quantum had been divided into a sufficient number of quanta. If this suggestion is correct, the beginning of the world happened a little before the beginning of space and time. I think that such a beginning of the world is far enough from the present order of nature to be not at all repugnant ... we could conceive the beginning of the universe in the form of a unique atom, the atomic weight of which is the total mass of the universe. This highly unstable atom would divide in smaller and smaller atoms by a kind of super-radioactive process.<sup>48</sup>

Lemaître's theory is as follows: if we use quantum theory to describe the universe, then the ultimate trajectory of the universe will be to achieve a state of maximum entropy. The quantum universe is made of a number of potential states.

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<sup>46</sup> Kragh (1987): 128.

<sup>47</sup> Eddington (1931c): 447-453; Kragh (1996): 130; Eddington (1928): 28; Godart (1992): 443; North (1994): 530; Whittaker (1949): 191.

Maximum entropy will require all these potential states to be occupied equally. This must then entail that, extrapolating backwards, we will reach a state of minimum entropy where the fewest possible potential energy states are occupied i.e. eventually to a single quantum. This is the primeval atom. The universe shrinks until everything is compressed inside one quantum of energy. Lemaître believed that you can extrapolate the expanding universe back to a single primeval quantum of energy. From this primeval atom, through some kind of radioactive decay, the universe will expand by dividing into smaller pieces. Lemaître sees the beginning of the universe as starting from the simplest form.<sup>49</sup>

Many astronomers found difficulty in reconciling the age of the universe given by Lemaître's theory with that given by stellar evolution. Astronomers found that the age of the earth exceeded that given for Lemaître's universe; which was cosmologically impossible. Lemaître addressed this problem by suggesting a 'fireworks theory' in which the universe exploded from his primeval atom to what it is now: 'The last two thousand million years are slow evolution: they are ashes and smoke of bright but very rapid fireworks.' The universe would be formed from the disintegration of the primeval atom, leaving behind remnants of this explosion as cosmic background radiation.<sup>50</sup>

Dissatisfaction of the Einstein-de Sitter universes led to Lemaître's creation of the expanding universe which involved the notion of time. This consequently brought the concept of the beginning of the universe into cosmology and with it implications of an initial singularity. Eddington, like Einstein before him, dismissed the idea and, although he supported Lemaître's model, constructed the Lemaître-Eddington

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<sup>48</sup> Lemaître (1931*b*): 706; Godart (1992): 444; Kragh (1987): 130.

<sup>49</sup> Heller (1996): 31-34. Heller, together with Godart, discovered a manuscript which Lemaître was preparing in the beginning of 1940, for the publication in a Japanese encyclopaedia, summarising his theory of the universe.

<sup>50</sup> Kragh (1987): 132.

cosmological model which conveniently put the beginning of his universe as starting from Einstein's model and tending towards de Sitter's model. Lemaître, unhappy with this interpretation, postulated a quantum beginning later adding the fireworks theory to explain the rapid expansion of his universe.

## **5.2 Bridging General Relativity and Quantum Mechanics**

The last and probably to Eddington the most important part of his career was spent trying to bridge general relativity and quantum mechanics. Dirac sprang his relativistic equation of the electron on an unsuspecting scientific world in 1928 using only special relativity combined with quantum mechanics. Dirac had worked alone and his new theory came as a shock to Eddington whose scientific world view was suddenly shattered. From the moment he was introduced to Dirac's theory, Eddington wholeheartedly threw himself into extending and completing what Dirac had begun. This eventually grew into an even bigger project which became his fundamental theory of the universe. Although Eddington continued to contribute to astrophysics and cosmology, his main scientific endeavour was reserved for his new project. The idea of trying to combine general relativity and quantum mechanics was the fundamental drive behind this new theory as well as the core argument against relativistic degeneracy. In this section, I will try and show that Eddington's reaction to Chandrasekhar's use of special relativity combined with Pauli's Exclusion Principle to produce relativistic degeneracy was influenced by his reaction to Dirac's equation.

### **5.2.1 Dirac's Relativistic Equation of the Electron**

One of the recurring accusations which have been levelled against Eddington is that he never really understood quantum mechanics. Clive W. Kilmister, who has

studied Eddington's fundamental theory and has written several articles and books on Eddington, said in an interview that Eddington's knowledge of quantum mechanics stemmed from the work of one of his research students, George Temple, who at the time was working on a problem set by Eddington to bridge the theory of relativity and quantum mechanics. He recalls,

The first three years I was at King's, George Temple was the head of department. And I once said to him, having been looking at Temple's book, a monograph called *An Introduction to Quantum Theory*, 'I think Eddington's idea of quantum theory was got from your book', and Temple who had been Eddington's research student at the time said, 'Oh, he never knew as much as that.'<sup>51</sup>

Although Eddington had read the relevant papers by Dirac, Heisenberg and Schrödinger, most of his understanding came from Temple's book. And Eddington in his usual way, as with general relativity, had reworked the theory in his own particular way. In fact, that 'the extent of his intimate knowledge was no more than Dirac's determination of the hydrogen energy levels. More complicated problems did not interest him at all, and he was convinced that their solution required a quite different approach; that is to say, the one that he was giving.'<sup>52</sup> We can also recall his confusion between the Bose-Einstein and Fermi-Dirac statistics and his reworking of the Pauli Exclusion Principle to fit his ideas of standing and progressive waves in describing degenerate electrons. This would support Kilmister's claim that Eddington's understanding of quantum mechanics was basic and what he called quantum mechanics was his own interpretation of the theory.

Eddington was not interested and played no part in the development of quantum theory. In fact, British scientists were on the whole uninterested in the field even though Jeans had written a report on quantum theory for the *Observatory* in 1914 (similar to

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<sup>51</sup> Kilmister interview (1997)

<sup>52</sup> Kilmister (1966): 2; Kilmister (1994): 88.

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Eddington's report on general relativity in 1918). The only other person at Cambridge who played an active role in the development and dissemination of quantum theory was Ralph Howard Fowler. Fowler was an expert on statistical mechanics who was highly interested in the new quantum theory of Heisenberg and Schrödinger and made frequent trips to Copenhagen to see Bohr.<sup>53</sup> He was the first to provide study sessions in quantum theory at Cambridge in his weekly colloquia at the Cavendish Laboratory and was also Dirac's doctoral supervisor in statistical mechanics.<sup>54</sup> Through his influence, Dirac later transferred his research interests to quantum theory and produced his seminal paper in 1928 on the 'Relativistic Equation of the Electron' which Fowler communicated to the *Royal Society*.

As discussed earlier in chapter two, Fowler had provided a solution for Eddington's paradox in his theory of white dwarf stars. Fowler's knowledge of quantum mechanics made him realise that a collapsing star can be stabilised by introducing the concept of electron degeneracy which will take over from radiative pressure, once all of the star's energy has been depleted, to balance the gravitational contraction and thus become a white dwarf.

By the end of 1926, many physicists believed that spin and relativity were 'intimately related.' The new quantum mechanics of Heisenberg, Schrödinger and Pauli did not agree with the picture of a point charge electron, as described by Bohr's old quantum theory, which gave rise to the duplexity phenomena where the observed number of states was twice the number given by theory. By this stage, the idea of an electron with spin angular momentum of half a quantum was already known. Dirac believes that 'the incompleteness of the previous theories [lies] in their disagreement with relativity, or, alternatively, with the general transformation theory of quantum

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<sup>53</sup> Kragh (1994): 7, 223-4, McCrea (1993); Milne (1945).

mechanics. It appears that the simplest Hamiltonian for a point charge electron satisfying the requirements of both relativity and the general transformation theory leads to an explanation of all duplexity phenomena without further assumption.<sup>55</sup> Dirac was not interested in finding a particular model for the electron, he was more interested in formulating a theory founded on general principles and considered the electron as a point charge. He wanted 'the interpretation of the relativistic quantum theory to be just as general as that of the non-relativistic theory.'<sup>56</sup> Dirac realised that quantum theory alone, as it stood, could not explain the discrepancies arising from the duplexity phenomena where 'the observed number of stationary states for an electron in an atom being twice the number given by the theory.'<sup>57</sup> What he found was that the relativistic consideration needed to be incorporated into the equation and that it was also connected to electron spin. This solved the duplexity problem.<sup>58</sup>

Dirac's paper focused on the relativistic wave equation of the electron but he did not use tensor calculus to derive it. Instead he used non-commutative algebra, a product of quantum mechanics. Dirac had, therefore, succeeded in establishing a bridge between relativity and quantum mechanics by producing a relativistic solution using quantum mechanical techniques. In his new theory, Dirac calculated the states of the hydrogen atom which will remain invariant under the Lorentz transformation. He also established the magnitude of electron spin to be  $\hbar/2$ , where  $\hbar = h/2\pi$  and  $h$  is Planck's constant, a fundamental unit for angular momentum. He also discovered the possibility of there existing negative energy, but not a negative probability, and thus postulated the existence of the positron. The true relativity wave equation should thus be such that its

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<sup>54</sup> Chandrasekhar Archive, Letter from Chandrasekhar to his father, 2 October 1930, Box 3/folder 2.

<sup>55</sup> Dirac (1928a): 610; Kilmister (1994): 90.

<sup>56</sup> Kragh (1994): 55-57; Dirac (1928a): 610.

<sup>57</sup> Dirac (1928a): 610.



solutions split up into two non-combining sets, referring respectively to the charge  $-e$  and the charge  $e$ .<sup>59</sup>

What was it about Dirac's paper that astounded Eddington? Kilmister suggests the following:

At first Eddington was content to see the two sides as simply two alternative ways of looking at the world, wholly independent of each other. He played no active role in developing the new quantum theory. But in 1928 Dirac's publication of his equation for the electron, an equation which was a natural development of the new quantum theory and yet was consistent with special relativity, alerted Eddington to the problem, a problem that he soon came to see as an opportunity. The realisation came in a personally painful way, for the equation contradicted a folk-belief, strongly held by Eddington amongst the majority, that relativity had in its possession a mathematical device (the tensor calculus) which could churn out all possible equations consistent with its tenets. Dirac's equation was not of this form.<sup>60</sup>

What most surprised Eddington was that the Dirac equation was not in the tensor form which he assumed was the fundamental form in which physical phenomena could be explained.<sup>61</sup>

From 1928, Eddington had actively sought for a combination of general relativity and quantum mechanics following Dirac's discovery of the relativistic equation for an electron which incorporated Einstein's special relativity into Schrödinger's wave equation. This accurately described electrons moving at speeds close to the speed of light. Until then no one had succeeded in using the two theories together. Kragh describes Eddington's reaction to Dirac's paper as being extremely impressed and that Eddington

elevated Dirac's equation to a status of universal significance and in a number of works applied his own version of the Dirac equation to

<sup>58</sup>The spin is a fundamental quantum property of a particle and is the intrinsic angular momentum. The electron is a spin-half particle and therefore electrons with different spin-half states have different intrinsic angular momentum and are considered as two individually different particles.

<sup>59</sup>Taylor (1987): 72-73. Phillips (2003):155; Dirac (1928a): 612. A mathematical discussion can be found in Kilmister (1966): 57-64.

<sup>60</sup>Kilmister (1994): x.

<sup>61</sup>Eddington (1923): 49; Kilmister (1966): 69; Kilmister (1994): 65, 101.

derive relationships between the macrocosmos and microcosmos, cosmic and atomic constants. Eddington believed that the Dirac equation did not describe an individual electron but instead gave the structural relation of the electron to the entire universe; indeed, in Eddington's philosophy of physics an 'individual electron' was a nonsensical notion.<sup>62</sup>

Eddington felt Dirac's equation was not a complete description as it used only special relativity. Dirac's equation was Lorentz invariant but was not consistent with general relativity.<sup>63</sup> His attempts to *unify* general relativity with quantum mechanics ran into difficulty and he later decided instead to build a theoretical bridge between the two, 'a harmonisation, rather than a unification, of relativity and quantum theory.'<sup>64</sup> Eddington changed his approach by trying to find a problem which could be solved by both general relativity and quantum mechanics and thus compare the two theories to find a common solution. Kilmister writes,

Everything changed, and not only for Eddington, in 1928 when Dirac published his wave equation for the electron, which was consistent with relativity and yet of a different form from any envisaged by the relativists. The new initiative prompted by Dirac's equation was what Eddington needed to make, as he thought, a break-through. Instead of a unified theory which would embrace both relativity and quantum mechanics, there was to be a bridge between them. The bridge would consist of certain problems having a particular simplicity, so that they could be treated by either method.<sup>65</sup>

Kilmister strongly suggests that the shock Eddington experienced when Dirac announced his theory later affected his acceptance of relativistic degeneracy. Chandrasekhar's theory was the point where Eddington's earlier astrophysical work first merged with his fundamental theory.<sup>66</sup> This may have been one of the factors which affected Eddington's reaction to Chandrasekhar's theory, although not the sole reason. Like Dirac's equation, Chandrasekhar's relativistic degeneracy formula used a

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<sup>62</sup> Kragh (1994): 225.

<sup>63</sup> Kilmister (1966): 70.

<sup>64</sup> Eddington (1936): preface.

<sup>65</sup> Kilmister (1994): 2.

combination of quantum mechanics in the form of degeneracy with *special* relativity. As shown previously, Eddington did not see this as a legitimate concept. Special relativity was only a partial and incomplete theory of relativity. Eddington felt that unless gravitational considerations were included, the theory could not accurately describe the universe. To Eddington, the theory was incomplete, and was only valid if *general* relativity was used. Eddington states in *Relativity Theory of Electrons and Protons*, in which he first presents his ideas that he later expands in his fundamental theory,

In 1928, P.A.M. Dirac made a bridge between quantum theory and relativity theory by his linear wave equation of the electron. This is the starting point of the development of relativity theory treated in this book.<sup>67</sup>

Until now, Eddington had believed that relativity theory was as ‘comprehensively and logically complete as a purely macroscopic theory had any right to be.’ Kilmister notes that ‘at this stage Eddington regards his work as a further extension of relativity, a ‘fourth step’ after the three steps of special relativity, general relativity and Weyl’s gauge theory.’<sup>68</sup> Eddington continues,

To say that Dirac’s wave equation was the first connecting link gives only a partial idea of its importance. *It was a challenge to those who specialised in relativity theory.* ... We had claimed to have in the tensor calculus an ideal tool for dealing with all forms of invariance and covariance. But instead of using the orthodox tool Dirac proceeded by a way of his own, and produced an expression of very unsymmetrical appearance, which he showed to be invariant for the transformations of special relativity theory. Why had this type of invariance eluded the ordinary tensor calculus? As C.G. Darwin put it, ‘it is rather disconcerting to find that apparently something has slipped through the net.’

Dirac’s object was to find a form of the electron equation (adhering to quantum theory) which should be invariant for rotations and Lorentz transformations. Eddington felt that the tensor calculations should have been able to cope with these calculations.

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<sup>66</sup> Kilmister (1994): 103-104.

<sup>67</sup> Eddington (1936): 1; Kilmister (1994): 93.

The failure of ordinary tensors to include Dirac's invariance is due to an arbitrary convention where the basic vector is identified with geometrical displacement  $(dx)^\mu$ . But by identifying the basic vector with Dirac's four-fold matrix  $\psi$ , a new tensor calculus can be formulated, the wave-tensor calculus.<sup>69</sup> Therefore Eddington needed to find a formula to express the old tensor form into the new wave-tensor form. He continues,

I was soon convinced that this was the extension of relativity theory for which we had been waiting, and that Dirac's equation was only the beginning of a more far-reaching application of the methods and conceptions of relativity theory to microscopic phenomena. After seven years' work I find the possibilities latent in the new departure still far from exhausted.<sup>70</sup>

Eddington wanted to construct a system using Dirac's 4-fold matrices, first introduced in his wave equation of an electron, into E-numbers which are designed to represent states which possess relativistic properties. The fundamental hypothesis of Eddington's theory is that in nature there exist equivalent 16-fold frames which can be appropriately represented by equivalent sets of E-numbers. Every 4-fold matrix represents an E-number.<sup>71</sup> He writes of his endeavour,

I am here limited by the fact that I do not propose to reinvestigate the whole quantum theory. I must develop the present relativity theory up to a point at which it meets the accepted results of quantum theory which are soundly (if unaesthetically) established. These results are given in matrix representation by Dirac and others, and the conventional nomenclature and definitions have reference to the matrix representations. I must have an eye on the theory that I am steering to meet before I actually make contact with it; therefore it seems unwise to postpone the transition to matrix representation for long. Meanwhile the knowledge that there is an equivalent theory in terms of general symbols is reassuring; for I cannot believe that anything so ugly as the multiplication of matrices is an essential part of the science of nature.<sup>72</sup>

And that,

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<sup>68</sup> Kilmister (1994): 93.

<sup>69</sup> Dirac (1928a): 614, Eddington (1936): 62-63.

<sup>70</sup> Kilmister (1994): 93, Eddington (1936):

<sup>71</sup> Eddington (1936): 36.

The reader must therefore be prepared to find here a greater elasticity in the definition and use of wave functions than he has been accustomed to.<sup>73</sup>

Eddington here states that his definition and use of wave function are different to that of ordinary quantum theory. We note again Eddington's reformulation of quantum mechanics in his own terms. Eddington does not accept the limitations in Dirac's theory which reduces the specification of a particle to a single wave-vector function. Eddington is very critical of Dirac's Lorentz invariant equation. He writes,

Current quantum theory neglects curvature of space; and therefore falls into error either way: either it postulates that a light relativistic object is used and wrongly neglects its uncertainty of position and velocity, or it postulates a heavy relativistic object and wrongly neglects the resulting curvature of space. Perhaps the most important insight obtained through a combination of relativity theory and wave mechanics, is a realisation that the two alternatives are different forms of the same error.<sup>74</sup>

Eddington constructs the Riemann-Christoffel matrix using general relativity tensors and wave functions from quantum mechanics where properties such as energy and momentum are reduced to a common form.<sup>75</sup> This, Eddington finds, corresponds to the de Sitter universe. When you specify some particles macroscopically you can get an alternative metric, the Einstein universe. It is more so de Sitter's universe because the Lorentz transformations from Dirac's theory are invalid in the Einstein universe as there is no motion.<sup>76</sup>

Once Eddington becomes involved in his investigation, he wants to distinguish between his and Dirac's theory, which he believes is incomplete, and he writes,

It may be well to make it clear that although the present theory owes much to Dirac's theory of the electron, to the general coordination of quantum theory achieved in his book *Quantum Mechanics*, and to the

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<sup>72</sup> Eddington (1936): 39.

<sup>73</sup> Eddington (1936): 66.

<sup>74</sup> Eddington (1936): 179.

<sup>75</sup> The Riemann-Christoffel tensor is the curvature tensor.

<sup>76</sup> Eddington (1936): 210.

many contributions of himself and others on these lines, it is not 'Dirac's Theory'; and indeed it differs fundamentally on most points which concern relativity. It is definitely opposed to what has commonly been called 'relativistic quantum theory', which, I think is largely based on a false conception of the principles of relativity theory.<sup>77</sup>

Kilmister believes that many physicists and cosmologists, as well as Eddington, thought that the union of quantum mechanics and relativity was very important but were unhappy with Eddington's treatment of the problem. In trying to build a unified theory of everything, Eddington was attacking the validity of quantum mechanics, which, many physicists felt, he did not fully comprehend.<sup>78</sup> By the latter part of his career, Eddington has rejected not only relativistic degeneracy, but the legitimacy of the Pauli Exclusion Principle with its requirement of the indistinguishability of particles and the Fermi-Dirac statistics which results from it. Kragh writes,

Eddington was criticized for his alleged idealism and his claim of being able to bridge cosmology and quantum theory. Most physicists felt that his interpretation of relativity and quantum theory was illegitimate. Dirac was no exception. In 1942, he felt obliged, together with Peierls and Pryce, to protest publicly against Eddington's critique of the standards in quantum mechanics. Referring to Eddington's objections against the customary use of Lorentz transformations, Dirac mildly corrected his senior colleague: 'The issue is a little confused because Eddington's system of mechanics is in many important respects completely different from quantum mechanics, and although Eddington's objections is to an alleged illogical practice in quantum mechanics he occasionally makes use of concepts which have no place there.'<sup>79</sup>

To this criticism, Eddington replies,

Reference has already been made to the erroneous (Stoner-Anderson) formula, which is currently used. In opposing my criticism of it, Dirac, Peierls and Pryce say: 'Eddington raises objections on similar grounds against the customary treatment of the equation of state of a degenerate gas. Here the situation is considerably simpler *because one*

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<sup>77</sup> Eddington (1936): 6.

<sup>78</sup> Kilmister (1994): 112.

<sup>79</sup> Kragh (1994): 227.

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*neglects the interaction between the particles altogether.*' That is why I reject altogether the customary treatment.<sup>80</sup>

Kilmister's explanation for Eddington's rejection of relativistic degeneracy is based on his analysis of Eddington's reworking of general relativity and quantum mechanics in order to extend Dirac's equation. He puts it down mainly to Eddington's fundamental shock in realising that all invariant equations were not of tensor form and that 'Eddington's whole intellectual framework was shattered' by this revelation.<sup>81</sup> Although Kragh does not analyse the Chandrasekhar-Eddington controversy itself he does delve into Eddington's foray in quantum mechanics and shows that many of his peers felt that by the time *Relativity Theory of Protons and Electrons* was published in 1936 Eddington's scientific career was essentially over.

Kilmister, as well as others writing about the controversy, have suggested that Eddington's rejection of Chandrasekhar's theory is partly due to jealousy of a much younger adversary trying to overthrow his theory, and his need to put Chandrasekhar in his place. Both Dirac and Chandrasekhar were in their mid-twenties when they challenged Eddington's views. Kilmister elaborates on this hypothesis suggesting that it was in fact Eddington's shock over Dirac's equation which influenced his action regarding Chandrasekhar's theory. Dirac's theory had put an enormous dent in Eddington's pride and his belief in the fundamental nature of relativity. Chandrasekhar's theory was an assault on Eddington's fundamental view of nature itself. That both should use special relativity, instead of general, combined with quantum mechanics was an additional insult. But Kilmister eventually rejects this as too simplistic an explanation to see 'Eddington at forty-nine as Master Builder still smarting under the shock of the

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<sup>80</sup> Eddington (1946): 92.

<sup>81</sup> Kilmister (1994): 101.

young Dirac's discovery and determined to withstand the next assault' from Chandrasekhar and continues,

The most generous explanation for Eddington's uncharacteristic behaviour would perhaps be that he was genuinely misled by the necessarily incomplete form of his new theory.<sup>82</sup>

Although Kilmister feels that Eddington's behaviour was uncharacteristic, we have seen that Eddington is not one to shy away from arguing a point which he believes intrinsically to be wrong. His controversy with Chandrasekhar mirrors that of his earlier controversies with Jeans and Milne in this instance. Kilmister's explanation is valid, but it does not fully answer why Eddington was so strongly against the limiting mass. There seems to be a greater reason and to find this, we need to understand Eddington's views on singularities.

### 5.2.2 The Fine Structure Constant and Gravitational Collapse

In the latter part of his career, Eddington was deeply involved in extending Dirac's relativistic wave equation of the electron and trying to put together a universal theory which combined general relativity and quantum theory. Apart from *Fundamental Theory* which was published posthumously in 1946, most of his early work in this area is contained in the *Relativity Theory of Protons and Electrons*. By the time *Relativity Theory of Protons and Electrons* was published in 1936, many of his peers felt that his theory had taken Eddington down an obscure scientific path which many found difficult to understand. Although his attempts were applauded many were baffled by his 'idiosyncratic interpretation of Dirac's wave equation of the electron'<sup>83</sup> leading one of his readers, Abdus Salam, the physicist and Nobel Laureate to recall,

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<sup>82</sup> Kilmister (1994): 104.

<sup>83</sup> Kragh (1987): 130.



I once unwisely criticised Eddington in Dirac's presence. My remarks were the result of exasperation with Eddington's *Fundamental Theory*. I believe I said that if Eddington were not a professor at Cambridge, he would not have had his book published. Dirac made the remark (which I have appreciated deeply later): 'One must not judge a man's worth from his poorer work; one must always judge him by the best he has done.'<sup>84</sup>

*Fundamental Theory* is a monumental work: obscure, difficult and a challenge to any reader. It is beyond the scope of this thesis to try and understand the work, but I will discuss one important aspect of Eddington's *Fundamental Theory* which is pertinent to the central question in this thesis. Is there a connection between Eddington's rejection of gravitational collapse and the fine structure constant about which Eddington was so obsessed?

In his investigation into Dirac's equation and a theory combining general relativity and quantum theory, Eddington finds seven constants of nature which are present in the universe. If you eliminate arbitrary units of length, time and mass, only four numerical constants remain which can be obtained purely by theoretical calculations, one of which was the fine structure constant of the hydrogen atom.<sup>85</sup> The fine structure constant is the constant that measures the strength of the electromagnetic interaction in quantum field theory. It is the 'fine structure' constant because instead of a single spectral line as with the circular orbits, a close-knit group of lines appear. And in Eddington's words, it is

conveniently described as the ratio of two units or 'atoms' of action. Such natural units are obtained when we multiply a separable element of energy by a time intrinsically associated with it. Corresponding to an atom of particle action there must be an atom of field action 137 times as great. Two well-known atoms of action are found experimentally to be in this ratio. In radiation phenomena a constant unit is obtained by multiplying the energy of a photon by the radian-period of the corresponding light-waves; the product is  $\hbar$ . In particle

<sup>84</sup> Taylor (1987): 90.

<sup>85</sup> Eddington (1936): 3, Kilmister (1994): 115. Eddington always used  $1/\alpha$  as the fine structure constant as opposed to just  $\alpha$ .

phenomena a constant unit is obtained by multiplying the energy  $e^2/r$  of an elementary electric doublet (electron and proton or positron) by the time equivalent  $r/c$  of the separation; the product is  $e^2/c$ . The ratio  $hc/e^2$  of these two units is found to be 137 with an experimental accuracy of about 1 part in 10,000.<sup>86</sup>

With this new research, Eddington's investigation slowly veered towards finding the origin of the charge of the electron and proton. Kilmister writes,

He came to see the algebraic structures arising from Dirac's original postulation as providing the clue to the union of relativity theory and quantum mechanics. Such a union was devoutly to be wished by many of Eddington's contemporaries in the 1930s. The difference between them and him arose because Eddington, on the analogy of Maxwell's unification of electricity and magnetism giving a value for the speed of light, expected and found values for other physical constants. I shall argue that initially these numbers arose for Eddington from a trial and error investigation of algebraic systems. As the investigation proceeded, Dirac's original equation, its later developments and Dirac himself moved into the background and the abstractions of algebra took over. Eddington's contemporaries gradually came to think that there was no more to it than that, that Eddington had convinced himself by manipulating the algebraic systems that certain accidental numbers were of great physical significance.<sup>87</sup>

Eddington's papers on the fine structure constant received a lot of attention from quantum physicists because 'it raised the hope of determining the size of the electron charge by means of a union of quantum theory and relativity.' The only problem was the obscurity of Eddington's arguments, and when the fine structure constant was found experimentally to be 137, rather than the originally thought 136, Eddington managed to manipulate his calculations so that he got 137. This raised a lot of sceptical eyebrows.<sup>88</sup>

Many physicists thought that relativity and quantum mechanics could be linked by a singularity, but Eddington was vehemently against this idea. To achieve gravitational collapse in quantum theory, Planck's constant  $h$  must tend to zero, but

<sup>86</sup> Eddington (1946): 38. Here  $\hbar$  is  $h/2\pi$  where  $h$  is Planck's constant,  $c$  is the speed of light,  $e$  is the particle energy and  $r$  is the radius of the particle.

<sup>87</sup> Kilmister (1994): 101.

<sup>88</sup> Kilmister (1994): 116. Eddington was obsessed with the fine structure constant. To Eddington, it grew in stature to become one of the main foundations underpinning the universe.

because the fine structure constant  $hc/2\pi e^2$  is invariant and cannot be zero, gravitational collapse has to be ruled out. What links quantum mechanics and general relativity may be a singularity, but this is only possible if Planck's constant  $h = 0$ , but as  $h$  is proportional to the fine structure constant, which have a value of 137, gravitational collapse cannot occur. The fine structure constant was one of the fundamental values in Eddington's cosmology and its existence could not be challenged.<sup>89</sup>

Here Eddington finally has a solid, quantitative argument against singularities. But because the theoretical foundation to his fundamental theory is obscure and, to many physicists, flawed, it is hard to assess whether Eddington's objection to singularities was because of the results from his theory or whether he found a way of manipulating his theory to validate his objection.

Eddington considered his final work as his *magnum opus*: a complete theory of the universe. But few, except perhaps for Lemaître, accepted or understood his work.<sup>90</sup>

Kragh succinctly describes Eddington's bewilderment at the reception of his theory:

Eddington was perplexed at the almost universal scepticism and indifference that met his theory. He felt that it was more than a bold speculation or imaginative hypothesis, and no more obscure than most of Dirac's contributions, the public success of which he seems to have envied. 'I cannot seriously believe that I ever attain the obscurity that Dirac does. But in the case of Einstein and Dirac people have thought it worthwhile to penetrate the obscurity. I believe they will understand me all right when they realize they have got to do so - and when it becomes the fashion "to explain Eddington,"' he complained in 1944.<sup>91</sup>

In his unpublished recollection of Eddington, Chandrasekhar gives a pertinent reason for Eddington's rejection of relativistic degeneracy and the important role which fundamental theory plays in this. In 1939, prior to a conference on white dwarfs, there was a discussion on relativistic degeneracy after dinner at Trinity College, Cambridge,

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<sup>89</sup> Durham (2003): 410.

<sup>90</sup> Heller (1996): 50.

with Dirac and Eddington. After discussing Eddington's argument regarding relativistic degeneracy, Dirac told Eddington that he did not agree with Eddington's argument.

Chandrasekhar recalls,

Eddington became very angry -- in fact, it was the only occasion when I saw him really angry. He got up from his chair, walked back and forth, and said, "The matter is not for joking!" ... Next day, after Hall, Eddington came up to me and said that he was very disappointed that Dirac did not seem to understand the implications of his own relativity theory of the electron. I did not assent or dissent with Eddington's remark but asked instead, "How much of your fundamental theory depends on your ideas on relativistic degeneracy?" He replied, "Why, all of it!" And since I did not react to that remark, he asked me why I had asked that question. My response was, "I am only sorry" -- not a polite remark to have made; but by that time I was really enraged with Eddington's supreme confidence in himself and in his own ideas.<sup>92</sup>

## 5.3 Singularities

It is becoming clear that singularities feature regularly in physics and especially mathematics, although they mostly appear in the form of mathematical constructs and are not generally considered physically real entities. Eddington himself studies them as hypothetical constructs in general relativity, cosmology and on his extension of Dirac's relativity equation of the electron, but he is secure in the knowledge that they were only mathematical models that were products of certain co-ordinate systems. This was generally the view taken by physicists and mathematicians at the time. Eddington's abhorrence of singularities was shared by many of his peers.

### 5.3.1 A Brief History of Singularities

Although the concept of singularities has been around for several hundred years, the term 'black hole' was coined only recently in 1967 by the American physicist John

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<sup>91</sup> Kragh (1994): 227.

<sup>92</sup> Chandrasekhar Archive, Addenda Box 77/folder 5.

Archibald Wheeler. As we shall see, the general outline of singularities and their effect on light remain constant throughout time, but the theoretical foundation of the theories and calculations undertaken by the scientists are markedly different. We can divide the history of singularity research into four different periods. In each of the three periods there is a significant paradigm shift in theoretical thought that was current during that period in history. We will divide the periods in the following way. The first occurs in the late 18<sup>th</sup> century when Newtonian physics and Newton's corpuscular theory of light is the accepted theoretical background. The second, 1916, occurs with the breakthrough of Einstein's theory of general relativity and the shift from Euclidean to non-Euclidean geometry.<sup>93</sup> The Newtonian theory of gravitation and the corpuscular theory of light are no longer acceptable. The third, the 1930s, occurs in a period of great change. Cosmology is no longer only a mathematical construct but a physically real one due to the discovery of Hubble's Law. With the formulation of quantum mechanics, classical physics no longer reigns supreme. General relativity is also no longer a completely new and alien theory, but has been taught in Cambridge for over a decade.<sup>94</sup> And the fourth, the 1960s to the present, occurs with a resurgence of interest in gravitational collapse and general relativity with the discovery of neutron stars. The advent of big science after the Second World War signals a change in the way science is conducted.

The term 'dark star' was first used by John Michell, a Rector from Thornhill, Yorkshire, in his 1783 paper on the effect of gravity on starlight delivered by his friend Henry Cavendish to the Royal Society to describe a star approximately 500 times the size of, but with the same density as, the sun. A Royal Society and Cambridge Fellow,

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<sup>93</sup> Euclidean geometry is two dimensional and flat whereas non-Euclidean geometry is curved. Positive curvature will be like that of a spherical surface where the combined angles of a triangle will be greater than 180°. Negative curvature is like a saddle and the combined angles of a triangle will be less than 180°.

<sup>94</sup> Warwick (2003): 482-3. Eddington began a series of introductory and later more mathematical, lectures on general relativity starting in the Lent term of 1920 at Cambridge. These lectures proved to be popular

Michell was a geologist, astronomer and also one of the founding fathers of seismology. His ideas were grounded in the two main theories of his time: Newton's theory of gravitation and the corpuscular theory of light. Michell realised that as light was thought to be composed of particles or corpuscles moving at a constant speed, it could, like other particles, be affected by gravity.<sup>95</sup> His calculations showed that for a star with the same density but with a radius 497 times that of our sun, the gravitational force was so large that the stream of light corpuscles would need a velocity greater than what it possessed in order to escape the star's surface.<sup>96</sup> There was a critical radius within which light could not escape the star, and such a star would not be visible: a dark star. 'This outrageous notion,' Gribbin writes, 'caused a major stir amongst the sober ranks of the fellows of the Royal Society.'<sup>97</sup>

In 1796 Pierre Simon de Laplace published his popular monograph on astronomy and cosmology, *Exposition du Système du Monde*, in which he independently tackled the idea of an escape velocity required for light to leave the sun. Laplace calculated that stars 250 times that of our sun will have a strong enough gravitational force to prevent light from escaping their surfaces: they will be invisible stars, or, as Laplace named them, 'des corps obscurs'. But the speed of light was already known to be constant and Laplace was unable to explain the paradox of light losing its velocity as

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and were delivered annually. General relativity was soon incorporated into the syllabus and became a standard part of the Mathematical Tripos exams.

<sup>95</sup> McCormach (1968): 126-128, 144; Schaffer (1979): 42-43; Israel (1987): 201-203; Gribbin (1990): 21-23; Gribbin (1996): 60; North (1994): 477; Thorne (1994): 122. See also Michell, J. (1784), 'On the Means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the velocity of their Light, in case such a Diminution should be found to take place in any of them, and such Data should be procured from Observations, as would be farther necessary for that Purpose.' *Phil. Trans.* 74: 35-57. Although Michell's ideas were first communicated officially in public in 1783, a year before the publication of his paper, he had been communicating some of his ideas unofficially to his friends for some time. The earliest reference to Michell's ideas are in Joseph Priestley's 1772 monograph, 'History and present state of discoveries relating to vision, light and colour.'

<sup>96</sup> The speed of light had already been accurately measured by the Danish astronomer Ole Rømer in 1675 using observation of eclipses of the moons of Jupiter to reveal how long light takes to cross the orbit of the Earth.

<sup>97</sup> Gribbin (1990): 21.

it slows down due to the gravitational force of the star. Consequently, he removed this problem from the third edition of his book.<sup>98</sup>

The theories of Michell and Laplace were formulated using the then current theories within classical physics: Newtonian gravitation and Newton's corpuscular theory of light. With growing interest within the scientific community towards a wave theory of light supported by Thomas Young's experiments on the interference of light in 1801, the corpuscular theory fell into disuse. And with it fell the hypotheses of dark stars. The effect of gravity on light as particles was not revived until after Einstein's formulation of the theory of relativity and, in particular, the 1919 solar eclipse expedition.<sup>99</sup>

It is not until 1916, when Karl Schwarzschild published two papers on general relativity in which he produces the exact solutions for Einstein's field equations, that we encounter the problem of dark stars again. Schwarzschild's research on the gravitational effect of a point mass in empty space and the gravitational field of a uniform sphere of matter came soon after Einstein's publication of his theory of general relativity in 1915. In fact, Schwarzschild wrote his papers while serving on the Eastern Front in the First World War and sent them to Einstein to communicate to the Prussian Academy of Sciences in Berlin. His calculations of Einstein's field equations revealed the curvature of space-time due to the gravitational field of any uniform sphere of matter.<sup>100</sup> Schwarzschild discovered that his curved space-time geometry predicted a critical radius of the sphere beyond which space curves so extremely that if the mass of the sphere is squeezed inside the critical radius, space would close around the mass and pinch off

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<sup>98</sup> Gribbin (1990): 24; North (1994): 477; Thorne (1994):123. What Mitchell and Laplace are describing is the escape velocity of a star. The escape velocity is the velocity required to overcome the gravitational pull of a star and escape its surface into space.

<sup>99</sup> Israel (1987): 204.

<sup>100</sup> Gribbin (1990): 59. This curved space-time geometry later became known as the Schwarzschild metric.

from the rest of the universe. The sphere would then be in a self-contained space (what we would now call a black hole) from which no light can escape. In other words, if a star collapses under its gravitational force, its radius will decrease until a certain distance from the centre of the sphere (the Schwarzschild radius) beyond which it can no longer emit radiation. This is the limiting radius beyond which even light cannot escape, a 'magic circle' as Eddington described it.<sup>101</sup> The conclusion Schwarzschild drew from his research into general relativity may be similar to that of the dark star postulated over a hundred years before, but unlike Michell and Laplace, Schwarzschild's theoretical foundation was completely different. By using Einstein's theory of general relativity as the basis for his work, he has abandoned classical physics with its reliance on Newtonian gravitation and the corpuscular theory of light favoured by Michell and Laplace. His is a completely new world view where gravity is no longer a force working at a distance but the curvature of space-time in which when the gravitational force of a star is very strong, the distortion of the curvature of the space surrounding the star will be extreme. However, Schwarzschild made it clear that this was a theoretical solution that was physically meaningless, a mathematical artefact, and did not pursue it any further.<sup>102</sup> Einstein, who communicated Schwarzschild's paper, found the idea of such a singularity problematic from the start and spent the rest of his career vigorously trying to get rid of it.

Fifteen years later in 1931, Stoner, Anderson and Chandrasekhar working independently discovered the limiting mass of white dwarf stars.<sup>103</sup> Unlike Schwarzschild however, they were not working in the field of general relativity, but in

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<sup>101</sup> The Schwarzschild radius is the radius of the event horizon around a black hole and is the distance from the centre of the black hole where the escape velocity is equal to the speed of light and is given by the equation  $R=2GM/c^2$  where  $G$  is the gravitational constant,  $M$  is the mass of the black hole and  $c$  is the speed of light. Eisenstaedt (1993): 353; Eddington (1920): 98.

<sup>102</sup> Israel (1987): 233; Eisenstaedt (1993): 353; Gribbin (1996): 62.

<sup>103</sup> See Chapter Two.



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the new field of stellar structure carved out by Eddington, Jeans and Milne. Fowler's contribution to the field was to add degeneracy to solve Eddington's paradox. To this mixture of astrophysics and quantum mechanics, Stoner, Anderson and Chandrasekhar added the special theory of relativity, not the general theory, to find the limiting mass. This is a completely different approach to that of Schwarzschild. The main focus here is special relativity and degeneracy, not gravity. The limiting mass dictated that stars above 1.4 times the solar mass would continue to collapse indefinitely. But to what, once again, no one would comment.

In the same year, Landau independently published a paper in which he found that stars bigger than 1.5 solar masses would collapse gravitationally to a singularity. But he concluded that since such 'ridiculous tendencies' were not observed, the stars probably contained 'pathological regions' in which the quantum mechanics laws failed, and dismissed the idea of such singularities.<sup>104</sup>

The discovery of the neutron in 1932 by James Chadwick at Cambridge, prompted astronomers to speculate on whether stars larger than 1.4 times the solar mass could become neutron stars. In 1939, Robert Oppenheimer and his student George Volkoff calculated the limiting mass of neutron stars to be 3 times the solar mass. Once again, stars with mass greater than the limiting mass of neutron stars would continue to collapse indefinitely.

Interest in relativistic degeneracy and gravitational collapse decreased with the advent of the Second World War which precipitated an increase in atomic and nuclear physics research. It was not until 1967 when pulsars, or rapidly rotating neutron stars,

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<sup>104</sup> Landau (1931): 367.

were discovered by Jocelyn Bell Burnell and her supervisor Anthony Hewish at Cambridge that interest in relativistic cosmology and singularities was rekindled.<sup>105</sup>

### 5.3.2 The Problem with Singularities

Many physicists were taken by surprise when the idea of the initial singularity first appeared. In order to deal with the concept of an expanding universe, one also had to tackle the problem of the beginning of the universe. Lemaître called the initial singularity the ‘zero value of the radius of the universe’ but he was hesitant to accept this as a physically real construct, and later wrote, ‘it is physically impossible for the volume of the universe to be strictly zero.’<sup>106</sup> In 1932 Lemaître showed that the Schwarzschild metric was not singular: it was not physically real, but fiction. It could be transformed away by changing the space-time coordinates.<sup>107</sup> He was aware that ‘from the physical point of view the singularity is a total catastrophe which makes all attempts to “prolong” the world’s history beyond the singularity highly hypothetical.’ He only brings back the idea once he read Eddington’s paper in *Nature* and tried to convince him about the primeval atom from which sprang Lemaître’s universe, not a singularity, a construct in which neither space nor time exists.<sup>108</sup> Although it may be inherent in the mathematics of the theory, Kragh writes, ‘Lemaître always emphasized that cosmology could and should be understood in physical terms, and he therefore denied that the beginning of the world could be represented by a true singularity or ‘annihilation of space’ as he called it. However, he also realised that the unwanted initial singularity is not easy to get rid of if the Friedmann-Lemaître equations are kept.’<sup>109</sup> Lemaître writes,

<sup>105</sup> Gribbin (1996): 63, 325.

<sup>106</sup> Eisenstaedt (1993): 365; Heller (1996): 29.

<sup>107</sup> Israel (1987): 235; Kerszberg (1989): 209; Eisenstaedt (1993): 365-366.

<sup>108</sup> Heller (1996): 15-18, 23.

<sup>109</sup> Kragh (1996): 54.

Whatever may be the philosophical interest in this behaviour it is not of great physical importance, as the behaviour of the universe before the instant of nearly zero radius would be entirely outside the field of any possible investigation. Furthermore we shall see later on that the astronomical evidence are in conflict with the collapsing type of motion. But even if the universe is of the everexpanding type it is not excluded to speculate, that this expansion has been preceded by the reverse motion, an evercontracting universe which has been burned to ashes and has rebound in the actual universe accessible to our observation.<sup>110</sup>

Lemaître is describing a cyclical model, or what became known as the 'Phoenix universe', where the universe starts with radius zero and goes back to zero and oscillates from that point.

Lemaître, like Landau, believed the field equations of general relativity broke down at very high densities. Lemaître was beginning to feel that relativity was insufficient to explain the expanding universe and that only quantum theory could provide the ideas. He thought the combination of general relativity and quantum theory, at that point in time, was highly unsatisfactory.<sup>111</sup>

Although the Lemaître universe was the main cosmological model in the 1930s, the physical aspects of the universe were never fully accepted. Many found the exploding primeval atom to be too fantastic and catastrophic. The beginning of the world, whether it was a singularity or a primeval atom, was too radical a concept to contemplate. At the end of 1932/beginning of 1933, Lemaître met Einstein at Caltech but 'Einstein was very sceptical about a singular beginning of the universe, in spite of his acceptance of the expansion of the universe.'<sup>112</sup>

As we have seen, Eddington's solution was not to shrink the universe to a singularity but to the Einstein universe. The Lemaître-Eddington universe will, therefore, expand from static equilibrium. 'Since,' Eddington explains,

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<sup>110</sup> Heller (1996): 30.

<sup>111</sup> Heller (1996): 50.

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I cannot avoid introducing this question of a beginning, it has seemed to me that the most satisfactory theory would be one which made the beginning not too unaesthetically abrupt. This condition can only be satisfied by an Einstein universe with all the major forces balanced.<sup>113</sup>

Eddington actually avoids saying anything about whether there was an abrupt beginning or not regarding the Lemaître-Eddington model. As long as there is an 'imperceptible and gradual beginning', he believes that will suffice for the theory. But Lemaître does not share Eddington's view of a gentle beginning; he prefers his 'fireworks theory'. Eddington continues,

I cannot think that my 'placid theory' is more likely to satisfy the general sentiment of the reader; but if he inclines otherwise, I would say – 'Have it your own way. And now let us get away from the Creation back to problems that we may possibly know something about'.<sup>114</sup>

We can see that Eddington really does not want to discuss the beginning of the universe. He does not even want to consider it, even as a scientific problem. He continues,

It is the opposite extrapolation towards the past which gives real cause to suspect a weakness in the present conceptions of science. The beginning seems to present insuperable difficulties unless we agree to look on it as frankly supernatural. We may have to let it go at that.<sup>115</sup>

Eddington's only concession to the idea of the beginning of the universe is his description of the heat death of the universe, when entropy reaches a maximum, and its reverse, the shrinking universe. He writes, 'All change is relative; and what we have called the theory of the 'expanding universe' might also be called the theory of the 'shrinking atom.' He describes what will happen to the universe in detail when entropy goes to zero, but he does not offer an opinion or suggestion as to whether this is a possibility, only that it ends with 'and then nothing.'<sup>116</sup>

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<sup>112</sup> Godart (1992): 445, 449.

<sup>113</sup> Eddington (1933/1958): 56.

<sup>114</sup> Eddington (1933/1958): 59.

<sup>115</sup> Eddington (1933/1958): 125.

<sup>116</sup> Eddington (1932):15; Eddington (1933/1958): 91-92. Chandrasekhar Archive Box 2/Folder 11. Handwritten reminiscence in which Chandrasekhar recalls one of Eddington's anecdotes where Eddington

One thing that has not been discussed before is Einstein's influence on Eddington regarding singularities. As we have seen, Einstein was not an ardent supporter of gravitational collapse. Since Schwarzschild's paper in 1916, he has had to contend with the monstrosity born from his general theory of relativity and tried hard to remove singularities from his model of the universe. His was a universe that did not change with time, there was no past or future. He was also quick to criticise Friedmann's solutions as soon as they were published and told Lemaître explicitly that even though the mathematics of the expanding universe was correct, the physical implications were unacceptable. Even though for a time in 1927 he was open to the idea of an expanding universe, he never wavered from his aversion to singularities. Eddington, likewise, was opposed to singularities from the beginning, since the Schwarzschild singularity was first conceived, and never changed his view. Could his complete acceptance of general relativity, and by association Einstein's view, have clouded his judgement regarding singularities? It is difficult to say for certain whether this was the case, but we cannot disregard the strong influence which Einstein exerted over Eddington. We must also not forget that singularities were universally abhorred by almost everyone, except for a few such as Milne, who explained their existence through religion. For many, it was a scientific Frankenstein's monster that could not be killed.

Eddington was aware of theories describing incredibly dense and collapsed objects (singularities) through the work of Laplace and Schwarzschild. But even though he was aware of them, he never believed that such objects could possibly exist. He was

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'had been the President of the Physical Society. And in his Presidential address he said "Instead of saying that the universe is expanding, we could equally say "we are all contracting."\* Faster and faster, smaller and smaller, one last blur of intense agitation and then nothing [\* we are all actors for the cosmic spectator]. The daily news came out with the headline "Sir Arthur says we are all getting smaller." A few days after this he received a letter which said "Dear Sir Arthur: I was very greatly pleased to read that an authority with your standing has said "We are all getting smaller." My brother who has been asserting this for years is forcibly retained in a mental hospital. Would you kindly help me in getting him released from the hospital.'"

aware of the possibility, but chose not to pursue an idea which he found abhorrent. In the first chapter of *Internal Constitution of the Stars* where Eddington surveys the state of astrophysics up to the publication of his monograph, he discusses the problem of giant stars. We find that he is describing Schwarzschild's solution discussed earlier. According to Einstein's theory of general relativity, if the density of a giant star increases, Eddington says the following will occur:

Firstly the force of the gravitation would be so great that light would be unable to escape from it, the rays falling back to the star like a stone to the earth. Secondly, the red-shift of the spectral line would be so great that the spectrum would be shifted out of existence. Thirdly, the mass would produce so much curvature of the space-time metric that space would close up round the star, leaving us outside (i.e. nowhere). The second point gives a more delicate indication and shows that the density is less than 0.001; for even at that density there would be a red-shift of the spectrum too great to be concealed by any probable Doppler effect.

Lest this argument should be regarded by our more conservative readers as ultra-modern, we hasten to add that it is to be found in the writings of Laplace –

'A luminous star, of the same density as the earth, and whose diameter should be two hundred and fifty times larger than that of the sun, would not, in consequence of its attraction, allow any of its rays to arrive at us' it is therefore possible that the largest luminous bodies in the universe may, through this cause, be invisible.'<sup>117</sup>

By using such phrases as 'rays falling back to the star like a stone to the earth', 'spectrum would be shifted out of existence' and 'space would close up round the star, leaving us outside (i.e. nowhere)', Eddington cleverly puts this problem into the realm of absurdity, i.e. the impossible. The phrases he use do not make sense; hence the concept of a giant star with high density will also not make sense. No doubt readers will chuckle at the notion. To make doubly sure that his readers understand the absurdity of such a star, he quotes from no less an authority than Laplace who stated the same thing almost two hundred years earlier. Eddington is underlining the fact that he is merely reiterating what has already been discussed and *dismissed* two hundred years earlier.

Laplace's quotation clearly describes a singularity: his 'invisible' star. But here, Eddington uses Laplace's words to illustrate what he, and by association Einstein, regards as a preposterous conclusion to what will happen if a giant star does not have low density. And it is also to emphasise that general relativity has also refuted the possibility of a dense giant star existing. Therefore, he does not have to pursue that problem in his research.

Chandrasekhar's limiting mass suggested that such objects may exist. To this, Eddington says,

I think there should be a law of Nature to prevent a star from behaving in this absurd way!<sup>118</sup>

Contrary to popular belief, there have been a number of attempts by mathematicians to probe the idea of gravitational collapse or black holes, although none were taken seriously until the 1960s.

## 5.4 Relativistic Degeneracy and Singularities

Although the main body of his stellar structure research was completed by the 1930s, it was not definitive and Eddington continued to think and write about it, especially on putting relativistic degeneracy to rest. By this time, he was also involved, as we have seen, in considering the implications of the cosmological models, which were emerging at the time, as well as extending Dirac's equation of the electron and constructing an acceptable theory incorporating general relativity and quantum mechanics (later to become his fundamental theory). It does not seem as though it was a simple decision to oppose relativistic degeneracy. In fact, it seems as though almost

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<sup>117</sup> Eddington (1926/1988): 6. The redshift here is due to the gravitational bending of light.

everything on which he was working in this period led, in some way, to the problem of singularities. No matter how hard Eddington tried, singularities were impossible to avoid.

We can only imagine Eddington's struggle with his earlier astrophysical work on white dwarfs when he discovered that they needed energy to cool down and stay compressed even when they had run out of energy (Eddington's paradox), his relief when Fowler provided him with electron degeneracy to solve the paradox only to be informed by Chandrasekhar that relativistic considerations had to be considered at such high electron velocities and that there was a limit beyond which the relativistic degeneracy pressure is no longer adequate. Eddington was then faced with the prospect of considering a situation in which the white dwarf would continue to contract indefinitely, precisely the same conclusion he was trying very hard to avoid in his cosmological work during this period.<sup>119</sup> In the *Relativity Theory of Protons and Electrons*, published in 1936, he discusses relativistic degeneracy and concludes,

The current theory ... gives an upper limit to  $\rho$  [density] only in the smaller stars; above a certain mass ... it would seem that as the star's energy supply gives out, it must go on contracting to ever higher density – until the space becomes so much curved that the terms 'contraction' and 'density' lose all meaning.<sup>120</sup>

Eddington's rejection of relativistic degeneracy is the crux of his opposition to Chandrasekhar's limiting mass. He did not accept the validity of the concept of the 'so-called relativistic degeneracy formula which has been widely but uncritically accepted'.<sup>121</sup> The reason he gives is that a combination of non-relativistic quantum mechanics and special relativity was not a *legitimate* theory because it was based on a *partial* theory of relativity.

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<sup>118</sup> Eddington (1935a): 38.

<sup>119</sup> Eddington (1926/1988): 172; Fowler (1926); Chandrasekhar (1931b), (1931d), (1935a).

<sup>120</sup> Eddington (1936): 255.



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If one takes the mathematical derivation of the relativistic degeneracy formula as given in astronomical papers, no fault is to be found. One has to look deeper into its physical foundations, and these are not above suspicion. The formula is based on a combination of relativity mechanics and non-relativity quantum theory, and I do not regard the offspring of such a union as born in lawful wedlock. I feel satisfied myself that the current formula is based on a partial relativity theory, and that if the theory is made complete the relativity corrections are compensated, so that we come back to the 'ordinary' formula.<sup>122</sup>

By the 'ordinary' formula, Eddington is referring to Fowler's non-relativistic degeneracy formula for the equation of state which results in all stars ending their existence as white dwarfs in the last stages of their evolution. Chandrasekhar's relativistic degeneracy formula, like the Stoner-Anderson formula, is specific to white dwarfs. It undermines Eddington's earlier attempts at establishing a stable end to a star's life.

Eddington's arguments regarding the invalidity of relativistic degeneracy utilises quantum mechanical considerations, which together with his flair for persuasion, convinced all that were present at the RAS meeting in January 1935 that Chandrasekhar's theory was conceptually flawed. It was not the mathematics that was at fault, he argued, it was the theoretical foundation of Chandrasekhar's research itself. To Eddington, this was a problem of greater magnitude than a miscalculation and so, he insisted, Chandrasekhar's theory should be dismissed. But were Eddington's intentions as straightforward as they seem?

Although Eddington's arguments against the limiting mass were couched in astrophysical terms, I believe that Eddington's bias against singularities, the impact of Dirac's equation and Eddington's subsequent research in cosmology and fundamental theory influenced his actions greatly during his controversy with Chandrasekhar. As we have seen, by the time Chandrasekhar publishes his first paper on relativistic degeneracy

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<sup>121</sup> Eddington (1936): 253.

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in 1931, Eddington has started working on cosmological problems presented by Lemaître's expanding universe. At the same time, he has already been trying to convert Dirac's relativistic theory of the electron into a more complete version by trying to combine general relativity, instead of special relativity, with quantum theory. His research on the combination of general relativity and quantum theory was leading him into investigating the fundamental constants of the universe which later formed the main body of his fundamental theory. *In each case, the problem of singularities kept appearing.*

In Lemaître's universe, Eddington briefly disposes of the singularity at the beginning of the universe by imposing Einstein's static universe as the point from which expansion begins. This Lemaître soon changes to that of the primeval atom of which, also because of the quantum theoretical implications which Lemaître relies on, Eddington is not convinced and does not want to accept. Lemaître does not accept the singularity for purely physical reasons, but Eddington does not want to discuss the problem of singularities at all.

In his work on Dirac's equation, Eddington finds that a singularity may be the missing link between general relativity and quantum theory, and that too, he finds abhorrent. Using his results regarding the fine structure constant, Eddington seems to have found a concrete point which shows that singularities cannot exist because the fine structure constant is a definite numerical constant.

This is all happening in the early 1930s, with singularities appearing in all the areas in which Eddington is involved. And then in 1935, Chandrasekhar finds the exact solutions for his limiting mass for which one of the possible solutions, for stars bigger than the limit, may be a singularity. And on top of this, Chandrasekhar's theory utilised

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<sup>122</sup> Eddington (1935):

the same theoretical tools used by Dirac in formulating his relativistic wave equation of the electron: quantum mechanics and *special* relativity.

There was no possible way for Eddington to have accepted the limiting mass. The years of research he had undertaken in trying to prevent the existence of singularities, whether it was the at the end of a star's life, the beginning of the universe or as the bridge between quantum mechanics and general relativity, would not allow him to accept such a conclusion, especially in his area of expertise, stellar structure. Here was a young upstart who had announced that he had found a solution to the problem of white dwarfs, a problem which Eddington believed to have already been solved nine years earlier in 1926, which predicted a singularity without discussing, or even understanding, the implications of such a solution. Eddington found no fault with the mathematics; it was the physical foundations of the theory which he found difficult to accept. As we have seen, these implications stretched back a long way, from Schwarzschild's initial discovery of singularities, Lemaître's universe and Dirac's equation. Even before this, Eddington understood and rejected the implications of Laplace's 'invisible' star.

The number of articles and books analysing the controversy in any depth is very small. Apart from Wali's biography of Chandrasekhar, there is only a handful that actually go to any length in explaining the controversy in terms of Eddington's motives. As we have seen, Kilmister tries to show that Eddington's rejection of relativistic degeneracy stemmed from his reaction against Dirac's relativistic equation of the electron and his subsequent research into his fundamental theory in his book *Eddington's Search for the Fundamental Theory*. Werner Israel's detailed study 'Dark Stars: An Evolution of an Idea', also tries to get to the root of Eddington's negative

reception of relativistic degeneracy and the limiting mass and provide an explanation.<sup>123</sup>

In his article, Israel points out that

if, as Eddington believed, Fowler's equation of state remains accurate at high densities, white dwarf radii would all be much larger than their Schwarzschild radii unless they happen to be more than 100 times heavier than the sun, and in the 1930s no stars as massive as this were definitely known to exist. Thus Eddington believed that no actual star would ever get into a situation where general relativistic effects could become significant.<sup>124</sup>

Israel also argues that even without the problem of singularities turning up as a conclusion of the Stoner-Anderson formula, Eddington would have rejected it due to his 'unorthodox definition of particle density which he proposed in his 1923 book, *The Mathematical Theory of Relativity*.' This unconventional definition increases the stability of massive stars by predicting a higher pressure for a given particle density by a factor of two and 'enshrined as it was in a major textbook, remained a source of confusion for years.' Eddington explains this discrepancy by attributing it to 'an extra potential or 'interchange' energy associated with an effective repulsive 'force' describing the action of the Pauli Exclusion Principle.<sup>125</sup> For an electron gas in its lowest quantum state, Eddington's equations will thus give Fowler's formula, not the Stoner-Anderson formula.<sup>126</sup> Throughout his explanation, Israel emphasises that Eddington's actual version is more complicated and was never completely understood by his peers and concludes,

I have gone into this point in painstaking detail because it seems to have gone generally unnoticed and because it reveals the underlying continuity and inner consistency of Eddington's thought over a time-span stretching well before the events of 1935. By his own account, the motivation which first caused him to question the Stoner-Anderson result was the 'stellar buffoonery' to which it led; but there can be

<sup>123</sup> Kilmister (1994); Israel (1987).

<sup>124</sup> Israel (1987): 219.

<sup>125</sup> Israel (1987): 220. Here Israel refers to Eddington (1923): 221 where Eddington argues that the particle density can be determined by the stress tensor  $T^{\mu\nu}$  in the equation  $nm = T^{\mu}_{\mu}$  where  $n$  is the magnitude of the particle flux vector and  $m$  is the particle rest mass.

<sup>126</sup> Israel (1987): 221.

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little doubt that the reason for his sustained opposition was grounded in purely technical considerations whose seeds went back more than a decade. Indeed, it is interesting that after its debut as the launch-pad of his 1935 paper, the contraction scenario and its 'absurdity' make no further appearance in his published work.<sup>127</sup>

Israel is correct in stating that Eddington's motivation has not been fully explained, and his argument regarding the root of Eddington's opposition to the Stoner-Anderson formula resting on his definition of particle density, although persuasive, is not complete. The 'stellar buffoonery' to which Eddington refers is Chandrasekhar's limiting mass, and it is true that until Chandrasekhar's exact calculations of his relativistic degeneracy equations were completed, Eddington did not in fact oppose the Stoner-Anderson formula. His letters to Stoner are encouraging with no severe criticisms regarding the formula. However, although Eddington's rejection of singularities has been consistent throughout the years, his objection to the relativistic degeneracy formula is not. Israel gives Eddington's definition of the particle density as the argument against the Stoner-Anderson formula for relativistic degeneracy. Yet against Chandrasekhar's paper on relativistic degeneracy, Eddington uses his arguments about progressive and standing waves. These are two conceptually different arguments. Eddington presents his arguments in technical terms because it is the only legitimate way to present his arguments to his peers, yet the core of his opposition is purely conceptual. As he had himself admitted, it is not Chandrasekhar's technical capabilities which he questions, but the conceptual foundation of his theory. And Israel is incorrect in his assumption that after 1935, Eddington did not publish anything on the absurdity of gravitational collapse. Eddington continued what can only be described as his crusade against relativistic degeneracy and the limiting mass, reiterating again and again in his papers and talks on white dwarfs that the 'current theory', meaning that in which the

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<sup>127</sup> Israel (1987): 221.

Stoner-Anderson relativistic degeneracy formula was used, was wrong and that his theory in which Fowler's formula was used was correct.<sup>128</sup> This would indicate that Eddington was aware that many of his colleagues had accepted the Stoner-Anderson formula and relativistic degeneracy, but he never gave up his opposition to the theory and took every opportunity provided to demonstrate this. By the mid-1930s Eddington's main astrophysical work was already eclipsed by his research in cosmology and his fundamental theory, which grew from his attempts at extending Dirac's relativistic equation of the electron. Israel acknowledges this saying,

By 1936 his unconventional stress tensor had so permeated his thinking on 'molar relativity' and Fundamental Theory that a change of course would have meant dismantling a complex interlocking structure.<sup>129</sup>

As we have already seen, when Chandrasekhar had asked Eddington how much of his fundamental theory depended on relativistic degeneracy, Eddington had replied, 'Why, all of it!'<sup>130</sup> Even if the technical aspect had satisfied Eddington, Israel does not believe that it would have been enough to make him accept relativistic degeneracy. The conservative nature of astronomy at the time and the lack of observational evidence would have prevented most astronomers and physicists from accepting the existence of singularities which 'even the arch-radical Landau at that time considered untenable.' Yet Israel feels that 'if the inner conviction was there, it is more than probable that he would have done so. The courage and integrity with which he defended his beliefs were legendary. Eddington's failure in 1935 was not a failure of nerve, but an aberration of a soaring imagination.'<sup>131</sup> Whether Eddington's failure was an aberration or not, we can agree that his 'inner conviction' opposed the idea of collapsing stars. This is an

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<sup>128</sup> Eddington (1936): 235, 246, 290, chapters 8 and 12; (1940); (1941); (1946): 89-92. By the mid-1930s Eddington's main astrophysical work was already eclipsed by his research in cosmology, his extension of Dirac's equation and his fundamental theory.

<sup>129</sup> Israel (1987): 220.

extremely interesting point, one which I strongly advocate. Eddington's intuition is notorious, most famously in his championing of general relativity. As Earman and Glymour notes, 'Eddington was committed to the theory before the expeditions were proposed; as he put it to Chandrasekhar, if the matter had been left to him, he "would not have planned the [eclipse] expeditions since he was fully convinced of the truth of the general theory of relativity."' <sup>132</sup>

Although Kilmister agrees that Israel has a valid point, he does not think Eddington's definition of particle density is the main reason why he rejected relativistic degeneracy. <sup>133</sup>

It is difficult to pinpoint exactly when his aversion to singularities began. The relativistic notion of singularities was first conceived by Schwarzschild after Einstein had completed his theory in 1915. But before that, there was the Newtonian concept of 'dark' or 'invisible' stars. Even though both come from different conceptual foundations, to Eddington, they were both unacceptable. He rejected them instinctively and aesthetically. Chandrasekhar has even accused Eddington of his supreme arrogance and belief in his understanding of nature. In an unpublished manuscript, Chandrasekhar writes,

I think the point really is that, men like Eddington - even people like Dirac or Einstein - at an early age in their careers, discover truths; truths which other people hadn't seen; and they somehow begin to get the feeling that they have away of deciding the validity or otherwise of a scientific statement, by their own perceptions, and that their perception has greater validity than anything anybody else may say. I have sometimes said that their having been enlightened at one time has blinded them to future perception. <sup>134</sup>

And in an interview in February 1980, Chandrasekhar says,

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<sup>130</sup> Chandrasekhar Archive, Addenda Box 77/folder 5.

<sup>131</sup> Israel (1987): 220.

<sup>132</sup> Earman and Glymour (1980): 84; Chandrasekhar (1975): 18.

<sup>133</sup> Kilmister interview.

For lack of a better word, there seems to be a certain arrogance toward nature which people develop. These people have had great insights and made profound discoveries. They imagine afterward that the fact that they succeeded so triumphantly in one area means they have a special way of looking at science which must therefore be right. But science doesn't permit that Nature has shown over and over again that the kinds of truth which underlie nature transcend the most powerful mind.

Take Eddington. He was a great man. He said that there must be a law of nature to prevent a star from becoming a black hole. Why should he say that? Just because he thought it was bad? Why does he assume that he has a way of deciding what the laws of nature should be?<sup>135</sup>

In all his articles and interviews on Eddington, Chandrasekhar never discusses the reasons behind Eddington's attack on relativistic degeneracy. To him the attack seemed incomprehensible and savage, and like many others, Chandrasekhar may have felt that by that stage, Eddington's involvement in his fundamental theory probably affected his scientific arguments.

## 5.5 Eddington the Quaker

A discussion of cosmology and singularities need not necessarily include religion. In Eddington's case, however, his strong Quaker faith and the various articles he wrote with a religious audience in mind, necessitate a discussion of whether Eddington's religious views influenced his science. It is especially interesting in this particular case of Eddington's rejection of singularities because the cosmological models constructed by Friedmann and Lemaître introduced the problem of the creation of the universe. Lemaître himself was accused of basing his expanding universe on his Catholic background, and thus introducing the concept of creation as a way of confirming his religious views. Up until then, the concept of creation had not cropped

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<sup>134</sup> Chandrasekhar Archive, Box 2/folder 11.



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up in the Einstein and de Sitter universes which were static. We will address the possibility of Eddington's rejection of singularities as inherent within his Quaker values and question whether we can necessarily attribute his scientific views to it.

Eddington was known as much for being a Quaker as well as one of the leading authorities in astrophysics and general relativity. His conscientious objection during the First World War, his continuous commitment to his religious duties and his public talks and articles on religion and science are well documented. He was a deeply religious man who, throughout his life, practiced his religion and lived according to its values.<sup>136</sup> Stanley discusses the Quaker work ethic which stresses a non-dogmatic approach to problems and critical scepticism. Since the 1880s, there has been a shift in the Quaker approach to science. Where before, subjects such as botany were popular, there was an increasing number of Quaker mathematicians studying at, and emerging from, Cambridge. A new modernist approach was gaining popularity among the educated Quakers and scholars were encouraged to question what had gone before and to seek new methods and interpretations; experience was of paramount importance. By the time Eddington came to study at Cambridge, almost twenty years has passed since this pedagogical shift and Eddington was one of the new breed of young mathematicians who were tackling their subjects within the Quaker work ethic. We can see in his early career that Eddington was not afraid of embarking on research projects in areas which were not the most popular or were completely new. His support of Einstein's general relativity, when hardly anyone in Britain had heard of Einstein, is not so surprising if you look at it from the point of view of his work ethic. His championing of Einstein's theory and the eclipse expedition which helped to establish it in 1919, as well as his well

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<sup>135</sup> Manuscript of Tierney, J (1980), 'Insights into the Universe', *Span* in Chandrasekhar Archive, Box 1/folder 10.

documented conscientious objection throughout the First World War, may have stemmed as much from his Quaker beliefs as well as his intuition regarding the truth of the theory.<sup>137</sup> If his Quaker beliefs so strongly influenced the way he conducted his work, could it also have influenced his rejection of singularities?

Eddington's early views about gravitational collapse since his reading of Laplace's book *Exposition du Système du Monde* have never wavered. He rejected the notion of singularities very early on and it stayed consistent throughout his career. We may even say that his stance was almost dogmatic regarding singularities. It does not seem as though Eddington rejected relativistic degeneracy and the limiting mass because it led to gravitational collapse, it is almost as though *because* of his instinctive rejection of singularities, the theory of the limiting mass, and hence relativistic degeneracy, was unacceptable to him from the beginning. It influenced his acceptance of relativistic degeneracy completely, and regardless of the validity of the mathematics or calculations, the fundamental premise on which the theory was based was, from the first, unacceptable to him.

Is there any substance to his arguments within a religious context? Did his initial rejection of singularities stem from his beliefs as a Quaker regarding the end of the world? This does not seem likely as Eddington frequently discusses the concept of heat death and supported the expanding universe. It was the notion of the beginning of the universe which he had trouble accepting, and he adjusted Lemaître's model of the expanding universe accordingly so that it began from a static Einstein universe. There is no direct evidence that indicates that Eddington's rejection of singularities came from a

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<sup>136</sup> Eddington (1930); Vibert Douglas (1953); Stanley (2003). See also Chandrasekhar (1987): 96; Earman and Glymour (1980); Wali (1992); McCrea (1991).

<sup>137</sup> Stanley (2003); Graham (1981): 76.

fundamental religious belief. In fact, rather than religious, it seems more likely that it was his scientific instinct which argued against their existence.<sup>138</sup>

Eddington is famous for his instinctive ability to recognise and to decide on the importance of theories. The most famous example is, of course, general relativity. When no one in Britain was interested, why was Eddington alone the champion of general relativity? Eddington went as far as to say that regardless of the outcome of the eclipse expedition, he knew that Einstein's theory was valid. Eddington's use of polytropes to describe stellar structure was also instinctive; there was no substantial evidence or earlier research which modelled stars as perfect gas spheres. In fact, one of Jeans' main arguments with Eddington was regarding Eddington's liberal use of assumptions when formulating his theory of stellar structure, and this was also the case with Milne. And finally, his work on the unification of general relativity and quantum theory was also instinctive. Having read Dirac's paper on the relativistic equation of the electron which only used special relativity, he knew there existed a bridge between the two theories and eventually this led to his work on his fundamental theory.

Eddington's instinct is a fascinating window into Eddington the scientist. He was a meticulous man who wrote everything down including all the books he read throughout the years. Yet when it came to his science, apart from the computations which had to be accurate, he made sweeping assumptions in all his theories. It would seem as though the bigger picture was more important than the path taken to achieve it. This enraged Jeans who believed that it was impossible to study stellar structure unless enough information was known about energy formation in stars. To Eddington, this was

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<sup>138</sup> In a private communication, Matthew Stanley, who has extensively researched the relation of Eddington's religious views to his science, states that he has not come across any evidence or indication that Eddington's rejection of singularities came from his religious views.

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not important so long as he knew that energy was being generated, and in the case of radiative transfer, it was found to be irrelevant.

It would seem that if we tried to put a religious slant to Eddington's reaction to relativistic degeneracy and singularities, we would fail. His views were more instinctive according to his scientific rather than religious world view.

But what about his views regarding cosmology and the beginning of the world? In a way, the beginning of the world in all the non-static models implied a singularity which neither Lemaître nor Einstein could remove. Eddington found this model of a beginning of Nature repulsive. This was generally the received view amongst astronomers and physicists at the time. Mathematicians regarded singularities as mathematical artefacts and, because they were not physically real, were therefore acceptable as one of many possible solutions to Einstein's field equations. But astronomers and physicists were more interested in creating a physically real model of the universe in which singularities presented a real problem in their physical as well as philosophical implications.

Could Eddington's strong dislike of singularities stem from his religious beliefs? He was meticulous about not including his religious beliefs in any of his scientific work, apart from popular expositions aimed at a religious audience. It is the same for Lemaître, but there were accusations regarding his creationist views in his cosmological work because of his vocation. Lemaître constantly had to deflect suspicion that his big bang theory originated in his religious views – that of creation. Einstein even 'thought that the singularity was due to the isotropy of the model studied and a reflection of the dogma of the creation in the thinking of a priest.'<sup>139</sup> The fact that Eddington was supporting his claims helped to popularise his work, yet because of Eddington's strong

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<sup>139</sup> Godart (1992): 445.

Quaker beliefs, some felt it tainted the science and was detrimental to its acceptance. Another blow, from which Lemaître unsuccessfully tried to distance himself, was the Pope's strong support of Lemaître's theory which put it in the context of God's creation which did not help its status within the scientific community. Although Eddington soon distanced himself from Lemaître's theory of the primeval atom and the fireworks beginning of the universe, the accusations never quite left him.<sup>140</sup>

Eddington always refrains from directly discussing the problem of singularities or the beginning of the universe, even though we see him describing what may happen when reversing the heat death and extrapolating backwards in time. This is in direct contrast to Jeans and Milne who, later in their careers, were not afraid to back their arguments using religion.<sup>141</sup> Eddington, on the other hand, starts his universe from the Einstein model, thus sidestepping the need to discuss creation.

Because there is no direct evidence in the scientific literature that Eddington rejected the idea singularities because of his religious beliefs, we cannot be certain that religion was a main factor in his scientific stance. It is possible that it may have influenced the way he *approached* the problem of singularities in the way that he always avoided discussing the issue in detail because he was aware of the religious implications that would entail, apart from declaring they were 'unthinkable', 'repugnant' and 'unaesthetically abrupt'. In fact Eddington made sure that there were no religious connotations in any of his scientific papers, precisely because he did not want to be accused of being biased and 'unscientific', especially since almost everyone in the

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<sup>140</sup> Deprit (1984): 387.

<sup>141</sup> Urani and Gale (1993); Kragh (1996): 66. By the 1930s most of astronomy's big names were involved, or were at least interested, in cosmology. Milne, who did not believe that general relativity was the definitive answer to modern physics or cosmology, formulated his own theory of the world, kinematic relativity, and extended this to include a theory of cosmology.

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scientific world was aware of his strong religious beliefs. The closest he gets to talking about God in his scientific work is when he talks about Nature.

In summary, the technical reasons behind Eddington's objection to relativistic degeneracy can be broken down into his objection to combining special relativity and quantum mechanics and his misunderstanding and reformulation of the Pauli Exclusion Principle and the Fermi-Dirac statistics which follow. But his conceptual rejection of relativistic degeneracy can be traced back further to his instinctive aversion to singularities which was already apparent by the time he became familiar with Schwarzschild's solutions to Einstein's field equations in 1917. By the time Chandrasekhar had completed his theory, Eddington found that singularities kept appearing in all the areas in which he was working: astrophysics, cosmology, general relativity and fundamental theory. Although they were separate subjects, they had this in common and he was unable to escape it.

The main reason for his objection to the limiting mass of white dwarfs is because of the implied existence of singularities which may result when this limit was exceeded. Before Chandrasekhar's exact theory was completed, Eddington was not really affected to such an extent by singularities because it was a *virtual* mathematical construct which could be taken care of simply by rearranging the coordinates and thus was not physically real.<sup>142</sup> Even in astrophysics, Eddington's paradox was resolved by Fowler and the Stoner-Anderson formula was only a mathematical tool which was not exact and had as its base Jeans' liquid stellar theory. By the time Chandrasekhar finished making his calculations to show that a limiting mass had to exist for all white dwarfs using Eddington's polytropic model, Eddington was encountering singularities in cosmology and fundamental theory. Because his fundamental theory placed special

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<sup>142</sup> Eddington (1923): 165; Eisenstaedt (1993): 358.

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emphasis on the fine structure constant which stipulated that singularities could not exist, Eddington felt he had concrete proof that this was so, and therefore relativistic had to be wrong. If Chandrasekhar's theory was correct, the basis of his fundamental theory would be wrong.

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## CONCLUSION

The aim of this thesis was to critically analyse the white dwarf affair within the context of the British scientific community in the early twentieth century and examine the controversy from the perspectives of both Chandrasekhar and Eddington. The core analysis was to have been on the technical aspect of the theory of the limiting mass, however, the Chandrasekhar-Eddington controversy was a deceptively technical controversy whose roots and mechanism were driven by issues deeper than stellar astrophysics, relativity, or quantum physics and were, to some degree, influenced by social factors. Up until now, references to the controversy have been mainly in a descriptive capacity and do not probe the reasons why Eddington could not accept relativistic degeneracy even though many of his peers began to accept the limiting mass towards the end of the 1930s. Many have attributed Eddington's actions to jealousy, intellectual obscurity and racism but these are highly speculative and superficial explanations and do not provide an in-depth analysis which may realistically explain why the controversy occurred. My analysis binds the technical, astrophysical side of the controversy with the social factors which were inherent in the scientific community at that period. There has also been no serious discussion of the Chandrasekhar-Eddington controversy in a historiographical context and I have attempted to do so in this thesis.

The controversy was examined in terms of its technical content but both Chandrasekhar and Eddington were not actually arguing over the validity of Chandrasekhar's calculations or his extension of Eddington's standard stellar model. It was over what the limiting mass implied: that singularities could exist. In order to scientifically argue that singularities were absurd, Eddington focussed on Chandrasekhar's *ad hoc* use of special relativity combined with Fowler's addition of



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electron degeneracy. The *ad hoc* nature of Chandrasekhar's theory was not the main reason for Eddington's dissatisfaction as Eddington himself was frequently accused of doing the same by Jeans and Milne. It was a convenient tool by which Eddington could demolish Chandrasekhar's theory and cast a pall over the validity of quantum mechanics with which he had been struggling in his research on his fundamental theory. Eddington's reasons ranged from the physical to the philosophical as he tried to construct a complete theory of the universe by calculating the fundamental constants from first principles.

I will conclude my analysis with the summary of the main points of the controversy and discussion of the possible reasons Eddington may have had in rejecting Chandrasekhar's theory and show that the locus of Eddington's arguments regarding his rejection of relativistic degeneracy centred on his instant rejection of, and inability to accept, the physical existence of singularities. I will also discuss how the controversy may be analysed in a historiographical context.

Over the years, many have questioned whether Eddington's reasons for starting the controversy with Chandrasekhar were purely scientific and not motivated by either his racial or religious views but I have found no evidence to support this. Chandrasekhar himself flatly denies that any racism was involved. Likewise with his religion, Eddington was scrupulous in keeping any religious overtones out of his scientific publications.

## **Summary of the Controversy**

We have seen that the scientific content of the controversy itself is technically straightforward: there is a limiting mass above which a star will not become a white

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dwarf. Chandrasekhar refrains from implying what will happen to stars once they exceed the limiting mass except to state that they will continue to contract, but he leaves the conclusion open. It is only later when Robert Oppenheimer constructs his theory of neutron stars and when black hole research flourished in the 1960s that singularities were beginning to be taken seriously. Chandrasekhar's sole purpose was to show that all stars do not settle down to become white dwarfs: there is a limiting mass above which a star will not find stability as a white dwarf and that there may be other possible outcomes to a star's life. In the public scientific arena, the controversy was argued solely over the technical method applied by Chandrasekhar. We have seen that Eddington's main objection was not Chandrasekhar's mathematics, but the conceptual foundation on which his theory was based. But this cannot be traced purely to relativity and astrophysics; Eddington had his own agenda, as did Milne. This encompassed their scientific work, but also involved their philosophical ideas and methodology. It also showed their psychological approach to their work, Eddington being driven by his instinct and Milne by Eddington's rejection earlier in his career. Chandrasekhar managed to avoid falling into the same trap as Milne by moving to Chicago and working in another field. Chandrasekhar could have also dragged out his controversy with Eddington if he had stayed on in England and moved in the same circles as Eddington and Milne. Apart from the extremely slim chance of an Indian getting an official teaching job at Cambridge, Chandrasekhar's decision to take the offer from the University of Chicago was also motivated by his reluctance to continue this controversy when he felt that Eddington was not open to persuasion. The lack of interest exhibited by his peers did not help matters either.

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As we have seen in chapter one, Eddington was no stranger to scientific controversies. As well as those with Jeans and Milne, Eddington was also involved in several other controversies throughout his career with physicists such as Sir Joseph Larmor, Sir Oliver Lodge and the philosopher Herbert Dingle. Eddington was a consummate debater, arguing over a variety of subjects over a number of years. The Chandrasekhar-Eddington controversy was not resolved until well after Eddington's death even though astronomers had unofficially accepted the validity of the limiting mass by 1939. Eddington's death put a period to the controversy.

The earlier controversies with Jeans and Milne provided the background to the Chandrasekhar-Eddington controversy, laying down the theoretical foundation for astrophysics in which the theory of white dwarfs could be investigated. White dwarfs puzzled astronomers for over twenty years until Eddington's polytropic stellar model and Fowler's introduction of electron degeneracy to stellar theory finally gave an explanation for their existence and stability. The main area of the Eddington-Jeans-Milne controversy which is directly relevant to this thesis is that of stellar structure. Eddington's polytropic model was constantly under attack from Jeans' liquid stars and Milne's degenerate core models, and this could also be seen in the white dwarf research undertaken by Chandrasekhar who used the polytropic model, Stoner who began his investigation using Jeans' liquid model and Milne's students who were using his composite and collapsed configurations. None of the astrophysicists were certain about the correct model, although Eddington's was considered the standard, but they somehow managed to create theories that seemed to produce suitable results. This did not mean that everyone was satisfied with the haphazard methods and the various assumptions used. As we have seen, Jeans and Milne were vociferous in their criticisms regarding

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Eddington's liberal use of assumptions as were physicists who were critical of astrophysicists in general.

The Chandrasekhar-Eddington controversy was firmly embedded in the Eddington-Jeans-Milne controversy that defined astrophysics in the 1920s and 1930s. In fact it was a more localised and specialised version of the earlier debates, centring on a particular area of stellar structure: white dwarfs. This is looking purely at the controversy from Chandrasekhar's point of view. However, if we examine the controversy from Eddington's point of view, we find that the canvas is even bigger. Chandrasekhar was just starting out in his academic career and the discovery of a limiting mass for white dwarfs was a big breakthrough, one which he thought would place him firmly in the league of Eddington, Jeans and Milne. He was aware that he was in the middle of their debate, in fact, that his theory might be a deciding factor in formalising the polytropic model as being the standard stellar model. Eddington, by this time, had been involved in general relativity, astrophysics, cosmology and was working on Dirac's equation by the time relativistic degeneracy was formulated. And by 1935, when Chandrasekhar had completed his exact theory, Eddington was already deeply involved in his fundamental theory. To Eddington, the limiting mass was not just about stellar structure. Relativistic degeneracy which led to the limiting mass and hence the possibility of singularities resonated deeply with problems he had been encountering in his other areas of research.

As we have seen, singularities have been appearing in all his research areas. Although they were not aesthetically pleasing and an abomination, Eddington managed to accept them as long as they were mathematical constructs and in no danger of being physically real. In general relativity, cosmology and his later research trying to bridge

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general relativity and quantum mechanics; they were seen only as mathematical solutions to problems. And all who produced them as solutions to Einstein's field equations vigorously refuted their physical existence, as we have seen with Schwarzschild, Einstein, Friedmann and Lemaître. When singularities began to appear in astrophysics with the use of the relativistic degeneracy formula, Eddington did not seem perturbed because the theory was only approximate. Stoner and Anderson did not comment on the implications of the mass limit and Landau stated that quantum mechanics was wrong. Chandrasekhar himself does not comment on the implications of his theory, but what he did was to make exact calculations to show precisely what the relativistic degeneracy formula indicated for all possible stellar configurations. Unlike the research that was done before, Chandrasekhar's theory now showed that singularities could physically exist. Up until this point, Eddington had managed successfully to avoid such a conclusion, but Chandrasekhar's theory showed that this was something which had to be addressed seriously. It was not just a mathematical construct or an approximation; it could actually exist. And if it did, it would also mean the possible collapse of his fundamental theory on which he had been working for the past seven years; it would negate the existence of the fine structure constant, one of the fundamental constants of the universe. Eddington was unable to accept this idea, and in order to argue against it, he focussed on relativistic degeneracy which, to him, was the central cause of this problem. Without relativistic degeneracy, white dwarfs could re-establish their equilibrium; there would be no limiting mass and no possibility of the actual existence of singularities. By concentrating on the technical aspect of the problem, Eddington could provide a legitimate way of showing that singularities could

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not possibly exist as opposed to announcing that it was intuitively unacceptable, even though this was probably Eddington's view.

## **Methodological Analysis**

The white dwarf affair was a controversy within a theoretical science and Warwick's constructivist approach seems the most appropriate method of analysis because of its aim to align the cultural history of theoretical science with that of experimental science such as in the studies of Bloor and Collins. Because the limiting mass controversy is theoretical, Bloor and Collins' methods of analysis are not as effective as Warwick's historiographical approach which is designed for a mathematically oriented science. It provides a social analysis without neglecting the theoretical content and addresses the problems inherent in trying to show that theoretical research does not occur in purely in contemplative solitude or as insights of genius but through a long process of training, discussion and debate, just as in the experimental sciences. A combination of the different methods is probably the best way of analysing the controversy. The Chandrasekhar-Eddington controversy was one of different cognitive aims and aesthetics and without analysing the social factors that influenced the controversy and its aftermath, it would not have been possible to understand why it took so long for the limiting mass to be officially accepted within the international astronomical community nor the reasons behind Eddington's actions. The controversy was not over mathematical ability or calculations, but methodology and, in Eddington's case, principle.

Controversies normally achieve closure through comparison of data, methodological and interpretive consensus by a jury of peers or, if consensus is

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unavailable, through a natural death or decrease of interest within the scientific community. But controversies which are over cognitive aims are more difficult to resolve due to their subjective and emotive nature which cannot be rationally negotiated. Because of this, resolution of the Chandrasekhar-Eddington controversy did not occur on the basis of scientific truth or consensus but through Eddington's death, even though the consensus of the astronomical and quantum physical communities by 1939 was that Chandrasekhar's theory was correct. As in the case of Eddington, even Dirac's criticisms were unable to make a difference. Eddington acknowledges that he is alone in his stance and laments the lack of recognition in his later work. He is aware of his isolation and the fact that the consensus of his community has swung towards Chandrasekhar, especially at his last public meeting with Chandrasekhar in 1939. Yet he doggedly pursues his path, unconvinced of the validity of relativistic degeneracy. By this time he is essentially alone in his opposition. In the eyes of the scientific community, the controversy has already closed with the acceptance of Chandrasekhar's limiting mass. But for Eddington, the controversy has not ended and he continued to write about the absurdity of relativistic degeneracy until his death in 1944.

This thesis was structured to analyse the technical aspect of the controversy but it could not fully do so without bringing in the social factors which influenced the astrophysicists' decisions. Like Collins' studies on the replication of scientific experiments, we can see that the methods scientists use and the facts and theories produced, whether in the laboratory or at the desk, are influenced by social factors and value judgements. We have also seen that there is a distinct difference in the way the 'core set' and the wider group of scientists involved reacted to the controversy. Eddington, Jeans and Milne were more concerned that priority was given in saving their

own theories and their opposition contributed enormously in diminishing the value of Chandrasekhar's research. The quantum physicists and other astronomers, such as Bohr, Dirac, Fowler and McCrea whom Chandrasekhar tried to enlist for support were reluctant to challenge Eddington's authority precisely because they were not in the 'core set'. Dirac only publicly challenged Eddington in 1941.

Bloor's principle of symmetry was also a useful tool because the controversy was multifaceted and the reasons behind it are complex. Simply probing the technical content of the controversy would not give a clear picture of its mechanism. It is not a question of which theory triumphed but why Chandrasekhar's theory was so violently opposed by Eddington when one would have expected them to have been on the same side, especially with their comparable educational training and abilities. We must remember here that although Chandrasekhar was trained in India, his education was almost identical in calibre to that of a Cambridge mathematician. He may not have sat the Mathematical Tripos nor had the coaching that was unique to Cambridge, which Eddington and Jeans' experienced, but the textbooks he studied, such as Eddington's *Internal Constitution of the Stars* and *Mathematical Theory of Relativity* and Sommerfeld's *Gaskugeln* and the journals he read, especially *MNRAS*, were also consistent with that of a Cambridge mathematician or astronomer at that time. Although Warwick's emphasis is on the local nature of the mathematical training (specifically in Cambridge), in the case of Chandrasekhar, the geographical location is not as important as the theoretical training method which may be spatially translated if the materials and methods used are the same. Rather than thinking of the training school as a physical entity in a specific location, one could perhaps think of the school as a collection of methods specific to Cambridge but not anchored in one location. Thus if one were to go



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through this school, one would emerge a Cambridge mathematician regardless of the geographical location. Chandrasekhar was also a member of the Indian elite: his father was a prominent civil servant and his uncle was a Nobel prize-winning physicist and had access to publications which may not have been available to others. Chandrasekhar was essentially educated as an Englishman. Chandrasekhar's confidence in his mathematical ability was almost as great as Eddington's precisely because his mathematical training was on par with other Cambridge mathematicians. His security in his ability comes from the tacit knowledge he acquired from his training as a mathematician unlike Stoner, who read Natural Sciences and had complained about the lack of mathematical training he received, and Milne, who never completed his Mathematical Tripos because of the onset of World War One.

Examining the controversy only from Chandrasekhar's perspective would automatically paint Eddington as the ruthless, authoritative teacher who would crush his student's first foray into the professional scientific arena and would be a one dimensional account. By also examining the controversy from Eddington's perspective the issues that were involved were found to be more than just technical but extended beyond the boundaries of astrophysics. Eddington's career has always been projected as a series of discrete episodes in his scientific career: astrophysics, relativity, cosmology and fundamental theory, but this thesis has shown that in one respect, with Eddington's consistent evasion and attempted elimination of singularities, there is a link between all areas of his research culminating in his search for a fundamental theory.

The effect of different working spaces such as the laboratory, scientific societies and universities play a large role in constructivist analyses of scientific controversies. The working space dictates the behaviour of the scientists and nowhere is it more

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obvious than in a controversy. Studies in experimental science have focussed on the laboratory as the main work space. For theoretical science, we do not have to constrict ourselves to the scientist's private study (even though it is the main research space for theoreticians), but can examine the collective areas of the colleges, the societies and private correspondence where exchanges in ideas occur. As we have seen, there is a difference in the mode of conduct in the public and private spheres. Within the collegiate atmosphere and private correspondence, the relationships between the astrophysicists were less formal and friendship and rivalry were more easily identified. In the public scientific arena, such as the RAS and in print, the astrophysicists were engaged in *professional* scientific debate and the tone is formal. Although superficial friendships are maintained, this is due more to the rules of professional conduct; it was seen as bad form to let private bitterness spill over into the public, scientific arena. It was a *sanitised* version of the science that went on in private.

Having applied a constructivist analysis to the controversy, we have been able to pry into the private exchanges between the scientists, their day to day conduct and witness the process of creating scientific knowledge. Astrophysics no longer seems a dry, precise, theoretical subject, but a living, evolving and human science. The analysis has also shown that there is a need to redefine scientific controversies to combine the philosophical, historical and sociological perspectives. The constructivist analysis has shown that controversies cannot simply be defined as experimental, theoretical, internal or external with appropriate closure mechanisms. There are different degrees of interpretation and resolution and each case must be analysed individually.

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## Concluding Remarks

The main objective of this thesis was to analyse the Chandrasekhar-Eddington controversy regarding the limiting mass of white dwarfs. As the background to the controversy and its aftermath were analysed, it became clear that there still remains a lot of critically unexamined material. The main source of mystery is Eddington himself. The more one delves into Eddington's work and his thoughts, the abyss between what we think Eddington believed in and what he really thought seems to grow wider. Although this thesis only touches on the many faceted areas of Eddington's research, we can see that the various strands of his work eventually culminate in his fundamental theory. It seems almost as though everything he had done previously was all a preparation for his ultimate theory of the universe. I have concentrated on relativistic degeneracy and the limiting mass of white dwarfs, but found that it played a major role in his views on singularities. The concept of singularities touched on almost *all* aspects of Eddington's research in astrophysics, cosmology, fundamental theory and general relativity. Spanning almost thirty years of concentrated research, the apex of his intellectual contribution and interest in all these subjects can be found in the early 1930s, precisely when Eddington's controversy with Chandrasekhar began.

## Appendix 1: The Stoner-Anderson Formula

### 1) Stoner's formula for a limiting density without relativistic corrections<sup>1</sup>

If we take the mean molecular weight to be  $2.5m_H$  where  $m_H$  is the mass of hydrogen, for a mass  $M$  of a sphere of radius  $r$  of uniform density,

$$M = \frac{4}{3} \pi r^3 (2.5m_H n) \quad (2.1)$$

and the total number of molecules in the mass

$$N = \frac{4}{3} \pi r^3 n = M / 2.5m_H \quad (2.2)$$

The mean kinetic energy for a degenerate electron is  $\frac{3}{40} (3/\pi)^{2/3} (h^2 n^{2/3})/m$ . Since the number of molecules is approximately equal to the number of free electrons and the kinetic energy of the nuclei is small, the total kinetic energy becomes

$$E_K = \frac{3}{40} (3/\pi)^{2/3} (h^2 n^{2/3})/m (M / 2.5m_H) \quad (2.3)$$

where  $h$  is Planck's constant and  $m$  is the mass of the electron.

The gravitational energy is

$$E_G = - aGM^2 / r \quad (2.4)$$

where  $G$  is the gravitational constant and  $a = (3/5) - n$ .

For uniform density,  $n = 0$  and  $a = 3/5$ . Thus

$$E_G = - (3/5)GM^2 / r = (- 3/5)(GM^2)(\frac{4}{3} \pi n)^{1/3}(2.5m_H)^{1/3}/M^{1/3} \quad (2.5)$$

if we substitute  $r$  for values of  $M$ ,  $n$  and  $m_H$  from (2.2).

The condition for a limiting density is therefore

$$d/dn (E_K + E_G) = 0 \quad (2.6)$$

where  $E_K$  is the kinetic energy,  $E_G$  is the gravitational potential energy and  $n$  is the number of electrons per unit volume and depends on the distribution of density.

From (2.4) and (2.3), we can write

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<sup>1</sup> Stoner (1929).

## Appendix 1: The Stoner-Anderson Formula

### 1) Stoner's formula for a limiting density without relativistic corrections<sup>1</sup>

If we take the mean molecular weight to be  $2.5m_H$  where  $m_H$  is the mass of hydrogen, for a mass  $M$  of a sphere of radius  $r$  of uniform density,

$$M = \frac{4}{3} \pi r^3 (2.5m_H n) \quad (2.1)$$

and the total number of molecules in the mass

$$N = \frac{4}{3} \pi r^3 n = M / 2.5m_H \quad (2.2)$$

The mean kinetic energy for a degenerate electron is  $\frac{3}{40} (3/\pi)^{2/3} (h^2 n^{2/3})/m$ . Since the number of molecules is approximately equal to the number of free electrons and the kinetic energy of the nuclei is small, the total kinetic energy becomes

$$E_K = \frac{3}{40} (3/\pi)^{2/3} (h^2 n^{2/3})/m (M / 2.5m_H) \quad (2.3)$$

where  $h$  is Planck's constant and  $m$  is the mass of the electron.

The gravitational energy is

$$E_G = - aGM^2 / r \quad (2.4)$$

where  $G$  is the gravitational constant and  $a = (3/5) - n$ .

For uniform density,  $n = 0$  and  $a = 3/5$ . Thus

$$E_G = - (3/5)GM^2 / r = (-3/5)(GM^2)(4/3 \pi n)^{1/3} (2.5m_H)^{1/3} / M^{1/3} \quad (2.5)$$

if we substitute  $r$  for values of  $M$ ,  $n$  and  $m_H$  from (2.2).

The condition for a limiting density is therefore

$$d/dn (E_K + E_G) = 0 \quad (2.6)$$

where  $E_K$  is the kinetic energy,  $E_G$  is the gravitational potential energy and  $n$  is the number of electrons per unit volume and depends on the distribution of density.

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<sup>1</sup> Stoner (1929).

$$\begin{aligned} & \text{and} \quad E_G = -\alpha n^{1/3} \quad \text{where } \alpha = (3/5) G M^{5/3} (4/3 \pi \times 2.5 m_H)^{1/3} \\ & \quad E_K = \beta n^{2/3} \quad \text{where } \beta = 3/40 (3/\pi)^{2/3} (h^2/m) M / 2.5 m_H \end{aligned} \quad (2.7)$$

$n$  is maximum when

$$(2.8) \quad 1/2 \alpha n^{-2/3} = 2/3 \beta n^{-1/3}$$

$$n = (\alpha / 2\beta)^3 \quad (2.9)$$

Substituting for  $\alpha$  and  $\beta$

$$n = 10^4 (\pi/3)^3 [G^3 M^2 m_H^4 m^3] / h^6 \quad (2.10)$$

Substituting the following values:

$$\begin{aligned} G &= 6.66 \times 10^{-8} & m_H &= 1.662 \times 10^{-24} \\ m &= 9.01 \times 10^{-28} & h &= 6.55 \times 10^{-27} \end{aligned}$$

$$\text{we get} \quad n = 2.31 \times 10^{-37} M^2 \quad (2.11)$$

In terms of solar mass  $M_\odot = 2.0 \times 10^{33}$

$$n = 9.24 \times 10^{-29} (M / M_\odot)^2$$

The maximum density

$$\begin{aligned} \rho &= 2.5 m_H^4 n \\ &= 3.85 \times 10^6 (M / M_\odot)^2 \end{aligned} \quad (2.12)$$

## 2) The Stoner-Anderson formula incorporating relativistic corrections<sup>2</sup>

In the revised formula incorporating relativistic corrections, what Stoner needs to modify is his expression for the total kinetic energy at absolute zero,  $E_K$ . For a non-relativistic treatment,

$$E_K = nV\varepsilon = nV \frac{3}{40} (3/\pi)^{2/3} h^2 n^{2/3} / m_0 \quad (2.13)$$

where  $\varepsilon$  is the mean kinetic energy of electrons.

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<sup>2</sup> Stoner (1930)

Therefore

$$E_K = nV c^2 (m - m_0) = nV 3/40 (3/\pi)^{2/3} h^2 n^{2/3}/m \quad (2.15)$$

$$p^2 = (m\beta c)^2 = m_0^2 c^2 [1/(1-\beta^2) - 1] \quad (2.17)$$
$$\begin{aligned} E_K &= (8 \pi V m_0^4 c^5 / h^3) [-x^3/3 + \int_0^x (1+y^2)^{1/2} y^2 dy] \\ &= (8 \pi V m_0^4 c^5 / h^3) [-x^3/3 + f(x)] \end{aligned} \quad (2.22)$$

For the white dwarf stars which Stoner is analysing, a complete expression for the integral is needed.

$$\begin{aligned} [f(x)]_{x<} &= \int_0^x (1+y^2)^{1/2} y^2 dy \\ &= \int_0^x (1+y^2)^{3/2} dy - \int_0^x (1+y^2)^{1/2} dy \end{aligned} \quad (2.23)$$

The first integral can be reduced by noting that

$$d/dy \{y (1+y^2)^{1/2}\} = 4(1+y^2)^{3/2} - 3(1+y^2)^{1/2} \quad (2.24)$$

The second integral is known. Thus the final result is

$$f(x) = 1/8 [x (1+x^2)^{1/2} \{1+2x^2\} - \log \{x + (1+x^2)^{1/2}\}] \quad (2.25)$$

The total kinetic energy thus becomes

$$\begin{aligned} E_K &= (8 \pi V m_0^4 c^5 / h^3) [1/8 x (1+x^2)^{1/2} \{1+2x^2\} - 1/3 x^3 \\ &\quad - \log \{x + (1+x^2)^{1/2}\}] \end{aligned} \quad (2.26)$$

$$E_K = (8 \pi V m_0^4 c^5 / h^3) f_1(x) \quad (2.27)$$

For the limit  $x \gg 1$ ,  $n \gg 5.88 \times 10^{29}$

$$\begin{aligned} (E_K)_{x \gg 1} &= (8 \pi V m_0^4 c^5 / 10 h^3) x^4 / 4 \\ &= 2 \pi V c p_0^4 / h^3 \\ &= n V 3/8 (3/\pi)^{1/3} h c n^{1/3} \end{aligned} \quad (2.28)$$

This shows that when  $n$  is large, the mean kinetic energy increases as  $n^{1/3}$ . Thus for a constant total number of electrons ( $nV$  is constant),  $E_K$  and  $-E_G$  vary as  $n^{1/3}$ . Under such conditions, equilibrium cannot be maintained.

For there to be equilibrium, there must be a limiting density. This will correspond to a value  $n$  when

$$d/dn (E_K + E_G) = 0 \quad (2.29)$$

If we substitute for  $x$  where  $x = p_0 / m_0 c$  from (2.21)

$$d/dx (E_K + E_G) = 0 \quad (2.30)$$

Taking  $2.5 m_H$  as the mean molecular weight, the gravitational potential energy is



$$E_G = -3/5 GM^2/M^{1/3} (4/3 \pi n)^{1/3} (2.5m_H)^{1/3} \quad (2.31)$$

Substituting  $(8\pi/3)^{1/3} m_0 c x / h$  for  $n^{1/3}$  from (2.21)

$$E_G = -3^{1/3} (4\pi/5)^{2/3} 1/h GM^{5/3} m_H^{1/3} m_0 c x \quad (2.32)$$

We know that  $E_K = (8\pi V m_0^4 c^5 / h^3) f_1(x)$  from (2.27).

Substituting  $M/(2.5m_H n)$  for  $V$  and  $n$  as above,

$$E_K = 3M m_0 c^2 / (2.5m_H) [f_1(x) / x^3] \quad (2.33)$$

Using the above in the equilibrium condition, we get

$$d/dx [f_1(x) / x^3] = 10^{1/3} (\pi/3)^{2/3} G m_H^{4/3} M^{2/3} / hc \quad (2.34)$$

Which becomes

$$d/dx [f_1(x) / x^3] = F(x) = 1.483 \times 10^{-23} M^{2/3} \quad (2.35)$$

$$M = 1.751 \times 10^{34} [F(x)]^{3/2} \quad (2.36)$$

$$F(x) = 1/8 x^3 [(3/x) \log \{x + (1+x^2)^{1/2}\} + (1+x^2)^{1/2} (2x^2 - 3)] \quad (2.37)$$

The mass can be found for any limiting electron concentration. As the mean molecular weight is  $2.5m_H$ , the limiting density is given by

$$\rho = 2.5m_H n = 4.15 \times 10^{-24} n \quad (2.38)$$

For very large  $x$ ,

$$\begin{aligned} F(x)_{x \gg 1} &= 1/8 x^3 (2x^3 - 3x) \\ &= 0.25 - 3/8 x^3 = 0.25 \end{aligned} \quad (2.39)$$

Therefore, the limiting mass for which equilibrium can be achieved is

$$\begin{aligned} M_0 &= 1.751 \times 10^{34} [0.25]^{3/2} \\ &= 2.19 \times 10^{33} \end{aligned} \quad (2.40)$$

If  $M_\odot = 2.0 \times 10^{33}$ , then  $M_0 = 1.095 M_\odot$ .

## Appendix 2: Chandrasekhar's theory for the limiting mass

### 1) Approximate theory - 1930

#### a) Ordinarily degenerate stellar configuration

We recall that the total pressure

$$P = p_r + p_g$$

where

$$p_r = (1 - \beta)P$$

$$p_g = \beta P$$

$\beta$  = ratio of gas pressure to radiation pressure.

In this instance,  $\beta = 1$  as we leave radiation pressure out and take the star to be an ideal case.

In a fully degenerate electron gas, the pressure

$$p_e = (\pi/60) h^2/m (3n/\pi)^{5/3}$$

and the number of electrons  $n = \rho / [\mu H(1 + f)]$

where  $\rho$  is the density of the stellar material

$f$  is the ratio of the number of ions to the number of electrons

$H$  is the mass of the hydrogen atom

$\mu$  is the molecular weight = 2.5 for fully ionised material

Therefore  $p_g = (\pi/60) h^2/m (3/\pi H)^{5/3} \rho^{5/3} / [\mu^{5/3} (1 + f)^{5/3}]$

$$= 9.845 \times 10^{12} [\rho / \mu (1 + f)]^{5/3}$$

Substituting  $K = 9.845 \times 10^{12} / [\mu (1 + f)]^{5/3}$

the total pressure  $P = K \rho^{5/3}$

Applying the theory of polytropic gas spheres where  $\kappa = 5/3$  or  $1 + 1/n = 5/3$  giving  $n = 3/2$

we get  $(GM/M')^{1/2} (R'/R)^{-3/2} = [5/2K]^{3/2} / 4\pi G$

Inserting values for  $M' = 2.7176$  and  $R' = 3.6571$  and taking the solar mass  $M_\odot = 1.985 \times 10^{33}$  we get

$$(M/M_\odot) R^3 = 2.14 \times 10^{28} / \mu^5$$

or  $R^6 \rho = 1.014 \times 10^{61} / \mu^5$

$$\rho = 2.162 \times 10^6 (M / M_{\odot})^2$$

### b) Relativistically degenerate stellar configuration

The density obtained in the previous paper was

$$\rho = 2.162 \times 10^6 (M / M_{\odot})^2$$

For a degenerate case where the number of electrons per cubic centimetre is greater than  $6 \times 10^{29}$ , the pressure of the gas will be

$$P = 1/8 (3/\pi)^{1/3} hc n^{4/3}$$

where  $h$  is Planck's constant  
 $c$  is the velocity of light

As  $n = \rho / [\mu H(1 + f)]$

We get  $P = K \rho^{4/3}$

where  $K = 3.619 \times 10^{14}$ .

Applying the theory of polytropic gas spheres where  $\gamma = 4/3$  or  $1 + 1/n = 4/3$  giving  $n = 3$ ,

we get  $(GM/M')^2 = (4K)^3 / 4\pi G$

For an ideal case with extreme degeneracy, the upper limit to the mass of an ideal white dwarf would be

$$M = 1.822 \times 10^{33} \\ = 0.91 M_{\odot}$$

[c.f. Stoner's limiting mass of  $2.2 \times 10^{33}$  or  $1.09 M_{\odot}$ ]

### 2) Exact theory - 1934<sup>3</sup>

For stellar material at a specified temperature  $T$  and density  $\rho$  we can define abstractly a quantity  $\beta$  denoting the ration between the gas pressure  $p$  and the total pressure  $P$  where  $P =$  sum of gas kinetic and radiation pressure.

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<sup>3</sup> Chandrasekhar (1934b).

Then if  $(960/r^4)(1-\beta)/\beta > 1$  (1)

the material at density  $\rho$  and temperature  $T$  will be a perfect gas in the classical sense.

If  $\beta_\omega$  be such that relation (1) is an equality then in stellar configurations in which  $(1-\beta)$  is always greater than  $(1-\beta_\omega)$  the stellar material continues to be a perfect gas however high the density may become. Degeneracy need not be brought into the equations.

If  $(1-\beta) > (1-\beta_\omega)$  then Eddington's polytropic equations are no longer enough and the equation of state for degenerate matter must be taken into account.

The equation of state for degenerate matter can be written in the following way:

$$\left. \begin{aligned} p &= (\pi m^4 c^5 / 3 h^3) f(x) \\ \rho &= (8 \pi m^3 c^3 \mu m_H) / 3 h^3 x^3 = B x^3 \end{aligned} \right\} \quad (2)$$

where  $f(x) = [x(x^2+1)^{1/2}(2x^2-3) + 3 \sinh^{-1} x]$  (3)

The pressure for a classical gas can also be written

$$p = (\pi m^4 c^5 / 3 h^3) [(960/\pi^4)(1-\beta_1)/\beta_1] 1/3 (2x^4) \quad (4)$$

For a completely collapsed configuration with  $\beta=1$ , the radiation pressure  $p'$  is zero and the total pressure  $p$  is given by (2). If we introduce the function  $\phi$  defined as

$$\rho = \rho_c (1 - 1/y_0^2)^{3/2} (\phi^2 - 1/y_0^2)^{3/2} \quad (5)$$

where  $y_0^2 = x_0^2 + 1$  and  $\rho_c = \rho_{\text{central}} = B x_0^3$  (5')

then the structure of the configurations is completely specified by the solution of the differential equation

$$(1/\eta)(d/d\eta)(\eta^2 d\phi/d\eta) = -(\phi^2 - 1/y_0^2)^{3/2} \quad (6)$$

with  $\phi = 1$  at  $\eta = 0$  and  $\phi(\eta_1) = 1/y_0$ ,  $\eta_1$  referring to the boundary (7)  
where  $\eta$  measures the radius vector in a suitable scale.

(6) is an exact equation; for a specified  $y_0$ , or central density, the structure is completely determined and in particular its mass. We see from (6) that as  $y_0 \rightarrow \infty$ ,  $\phi \rightarrow$  the Emden function with index 3. The mass of these configurations therefore tends to a unique limit as  $y_0 \rightarrow \infty$ . This mass is  $M_3$  or the limiting mass. Configurations with mass less than  $M_3$  will have a finite radii.

When  $\beta_1$  has the same value in the envelope as in the core then only collapsed configurations are possible i.e. a composite configuration has a  $1 - \beta$  which is always less than the value  $(1 - \beta_1)$  which it has in the wholly gaseous state. The nature of the curves of constant mass can at once be predicted. If the mass is less than  $M_3$ , then in the completely collapsed state ( $\beta_1=1$ ) it has a unique radius. For each mass  $M'$ , it is possible to calculate  $\beta_M$ .

$\beta_{M_3}$  can be specified by  $(960/\pi^4)(1 - \beta_{M_3} / \beta_{M_3}) = 1$

If  $\beta_{M_3} = \beta_0$ , then  $(1 - \beta_0) > (1 - \beta_0)$ .

For stars with  $M_3 < M \leq M$ , when  $(1 - \beta) < (1 - \beta_0)$  then the configuration has a mass  $M_3 \beta^{3/2}$  as  $y_0 \rightarrow \infty$ ,

and hence  $M = M_3 \beta^{3/2}$

where  $\beta = (\pi^4/960) [\beta^\dagger^4 / (1 - \beta^\dagger)]^{1/3}$  where  $\beta^\dagger$  is the value in the wholly gaseous state.

$\beta^* = \beta^\dagger = \beta_0$  is a solution and  $\beta^* = 1$  when  $\beta^\dagger = \beta_0$ .

And we get  $M = M_3 \beta^{3/2}$ .

What Chandrasekhar did basically was to find the differential equation (6) which will give exact solutions that can be plotted to show the mass-radius relationship for different configurations. This equation is as follows:

$$(1/\eta^2) d/d\eta (\eta^2 d\phi/d\eta) = -(\phi^2 - 1/y_0^2)^{3/2}$$

To describe the inner relativistically degenerate core we need an Emden function of index 3. Index 3/2 means a degenerate core.

From (6) we see that  $\phi \rightarrow \theta_3$ ,  $y_0 \rightarrow \infty$ . At the same time the radius tends to zero.

We see that

$$M \rightarrow -4\pi(2A^2/\pi G)^{3/2} 1/B^2 (\xi^2 d\theta_3/d\xi)_1$$

Where the relativistically degenerate constant

$$K_2 = (3/\pi)^{1/3} hc/8(\mu H)^{4/3}$$

Is related to  $A^2$  and  $B$  by the relation a

$$K_2 = 2A^2/B^{4/3}$$

What Chandrasekhar found was that there was a mass limit

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$$M_{\text{limit}} = 0.197 (hc/G)^{3/2} 1/(\mu_e H)^2 = 5.76 \mu_e^{-2} \odot$$

Which he found to be approximately  $1.44 \odot$ .

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